PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCIX.

PART I.

LONDON,

PRINTED BY W. BULMER AND CO. CLEVELAND-ROW, ST. JAMES'S;
AND SOLD BY G. AND W. NICOL, PALL-MALL, BOOKSELLERS TO HIS MAJESTY,
AND PRINTERS TO THE ROYAL SOCIETY.

MDCCCIX.

PHILOSOPHICAL TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

LONDON,

FOR THE YEAR MDCCCIX.

PART I.

LONDON,

PRINTED BY W. BULMER AND CO. CLEVELAND-ROW, ST. JAMES'S;
AND SOLD BY G. AND W. NICOL, PALL-MALL, BOOKSELLERS TO HIS MAJESTY;
AND PRINTERS TO THE ROYAL SOCIETY.

MDCCCIX.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to

276 80

be, the importance and singularity of the subjects, or the advantageous manner of treating them, without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

CONTENTS.

I. *The Croonian Lecture. On the Functions of the Heart and Arteries. By* Thomas Young, *M. D. For. Sec. R. S.* page 1

II. *An Account of some Experiments, performed with a View to ascertain the most advantageous Method of constructing a Voltaic Apparatus, for the Purposes of Chemical Research. By* John George Children, *Esq. F. R. S.* p. 32

III. *The Bakerian Lecture. An Account of some new analytical Researches on the Nature of certain Bodies, particularly the Alkalies, Phosphorus, Sulphur, Carbonaceous Matter, and the Acids hitherto undecompounded; with some general Observations on Chemical Theory. By* Humphry Davy, *Esq. Sec. R. S. F. R. S. Ed. and M. R. I. A.* p. 89

IV. *An Account of a Method of dividing Astronomical and other Instruments, by ocular Inspection; in which the usual Tools for graduating are not employed; the whole Operation being so contrived, that no Error can occur but what is chargeable to Vision, when assisted by the best optical Means of viewing and measuring minute Quantities. By Mr.* Edward Troughton. *Communicated by the Astronomer Royal.* p. 105

V. *A Letter on a Canal in the Medulla Spinalis of some Quadrupeds. In a Letter from Mr.* William Sewell, *to* Everard Home, *Esq. F. R. S.* p. 146

VI. *A numerical Table of elective Attractions; with Remarks on the Sequences of double Decompositions.* *By* Thomas Young, *M. D. For. Sec. R. S.* page 148

VII. *Account of the Dissection of a Human Fœtus, in which the Circulation of the Blood was carried on without a Heart.* *By Mr.* B. C. Brodie. *Communicated by* Everard Home, *Esq. F. R. S.* p. 161

VIII. *On the Origin and Formation of Roots.* *In a Letter from* T. A. Knight, *Esq. F. R. S. to the Right Hon. Sir* Joseph Banks, *Bart. K. B. P. R. S.* p. 169

IX. *On the Nature of the intervertebral Substance in Fish and Quadrupeds.* *By* Everard Home, *Esq. F. R. S.* p. 177

APPENDIX.

Meteorological Journal kept at the Apartments of the Royal Society, by Order of the President and Council.

THE PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged the Medal on Sir GODFREY COPLEY's Donation, for the year 1808, to WILLIAM HENRY, M. D. for his various Papers communicated to the Society, and printed in the Philosophical Transactions.

And they adjudged the Gold and Silver Medals, on the Donation of BENJAMIN COUNT of RUMFORD, to Mr. WILLIAM MURDOCH, for his publication of the employment of Gas from Coal, for the purpose of Illumination.

PHILOSOPHICAL TRANSACTIONS.

I. *The Croonian Lecture. On the Functions of the Heart and Arteries.* By Thomas Young, *M. D. For. Sec. R. S.*

Read November 10, 1808.

THE mechanical motions, which take place in an animal body, are regulated by the same general laws as the motions of inanimate bodies. Thus the force of gravitation acts precisely in the same manner, and in the same degree, on living as on dead matter; the laws of optics are most accurately observed by all the refractive substances belonging to the eye; and there is no case in which it can be proved, that animated bodies are exempted from any of the affections to which inanimate bodies are liable, except when the powers of life are capable of instituting a process, calculated to overcome those affections, by others, which are commensurate to them, and which are of a contrary tendency. For example, animal bodies are incapable of being frozen by a considerable degree of cold, because animals have the power of generating heat; but the

skin of an animal has no power of generating an acid, or an alkali, to neutralise the action of an alkaline or an acid caustic, and therefore its texture is destroyed by the chemical attraction of such an agent, when it comes into contact with it. As far, therefore, as the functions of animal life depend on the locomotions of the solids or fluids, those functions must be capable of being illustrated by the consideration of the mechanical laws of moving bodies; these laws being fully adequate to the explanation of the connexion between the motive powers, which are employed in the system, and the immediate effects, which they are capable of producing, in the solids or fluids of the body: and it is obvious, that the inquiry, in what manner, and in what degree, the circulation of the blood depends on the muscular and elastic powers of the heart and of the arteries, supposing the nature of those powers to be known, must become simply a question belonging to the most refined departments of the theory of hydraulics.

In examining the functions of the heart and arteries, I shall inquire, in the first place, upon the grounds of the hydraulic investigations which I have already submitted to the Royal Society, what would be the nature of the circulation of the blood, if the whole of the veins and arteries were invariable in their dimensions, like tubes of glass or of bone; in the second place, in what manner the pulse would be transmitted from the heart through the arteries, if they were merely elastic tubes; and in the third place, what actions we can with propriety attribute to the muscular coats of the arteries themselves. I shall lastly add some observations on the disturbances of these motions, which may be supposed to occur in different kinds of inflammations and of fevers.

When we consider the blood vessels as tubes of invariable dimensions, we may suppose, in order to determine the velocity of the blood in their different parts, and the resistances opposed to its motion, that this motion is nearly uniform, since the alternations, arising from the pulsation of the heart, do not materially affect the calculation, especially as they are much less sensible in the smaller vessels than in the larger ones, and the principal part of the resistance arises from these small vessels. We are to consider the blood in the arteries as subjected to a certain pressure, by means of which it is forced into the veins, where the tension is much less considerable; and this pressure, originating from the contractions of the heart, and continued by the tension of the arteries, is almost entirely employed in overcoming the friction of the vessels: for the force required to overcome the inertia of the blood is so inconsiderable, that it may, without impropriety, be wholly neglected. We must therefore inquire, what the magnitude of this pressure is, and what degree of resistance we can suppose to arise from the friction of the internal surface of the blood vessels, or from any other causes of retardation. The magnitude of the pressure has been ascertained by HALES's most interesting experiments on a variety of animals, and may thence be estimated with sufficient accuracy for the human body; and for determining the magnitude of the resistance, I shall employ the theorems which I have deduced from my own experiments on very minute tubes, compared with those which had been made by former observers under different circumstances; together with some comparative experiments on the motion of water and of other fluids in the same tubes.

Dr. HALES infers, from his experiments on quadrupeds of different sizes, that the blood in the human arteries is subjected to a pressure, which is measured by a column of the height of seven feet and a half: in the veins, on the contrary, the pressure appears to amount to about six inches only: so that the force which urges the blood from the greater arteries through the minuter vessels into the large veins, may be considered as equivalent to the pressure of a column of seven feet.

In order to calculate the magnitude of the resistance, it is necessary to determine the dimensions of the arterial system, and the velocity of the blood which flows through it. According to the measurements of KEILL and others, we may take $\frac{1}{4}$ of an inch for the usual diameter of the aorta, and suppose each arterial trunk to be divided into two branches, the diameter of each being about $\frac{4}{5}$ of that of the trunk, (or more accurately $1 : 1.26 = 10^{-.100567}$), and the joint areas of the sections about a fourth part greater, (or $1.2586 : 1 = 10^{.099896}$). This division must be continued twenty nine times, so that the diameter of the thirtieth segment may be only the eleven hundredth part of an inch, that is, nearly large enough to admit two globules of the blood to pass at once. The length of the first segment must be assumed about nine inches, that of the last, the twentieth of an inch only; and supposing the lengths of the intermediate segments to be a series of mean proportionals, each of them must be about one sixth part shorter than the preceding, (or $1 : 1.961 = 10^{-.07776}$), the mean length of the whole forty six inches, the capacity to that of the first segment as 72.71 to 1, and consequently the weight of the blood contained in the arterial system about 9.7

pounds. It is probable that this calculation approaches sufficiently near to the truth: for the whole quantity of blood in the body being about 40 pounds, although some have supposed it only 20, others no less than 100, there is reason to believe that half of this quantity is contained in the veins of the general circulation, and that the other half is divided, nearly in equal proportions, between the pulmonary system and the remaining arteries of the body, so that the arteries of the general circulation may contain about 9 or 10 pounds. HALLER allows 50 pounds of circulating fluid, partly serous, and partly red, and supposes $\frac{1}{5}$ of this to be contained in all the arteries taken together: but in a determination which must be in great measure conjectural we cannot expect perfect accuracy: and according to HALLER's own account of the proportions of the sections of the arteries and veins, the large trunks of the veins appear to be little more than twice as capacious as those of the arteries, and the smaller branches much more nearly equal, so that we cannot attribute to the arterial system less than $\frac{1}{3}$ of the whole blood.

It may be supposed that the heart throws out, at each pulsation, that is about seventy five times in a minute, an ounce and a half of blood: hence the mean velocity in the aorta becomes eight inches and a half in a second: and the velocity in each of the succeeding segments must of course be smaller, in proportion as the joint areas of all the corresponding sections are larger than the area of the aorta: for example, in the last order of vessels, of which the diameter is the eleven hundredth of an inch, the velocity will be one ninety third of an inch: and this result agrees sufficiently well with HALES's observation of the velocity in the capillary arteries of a frog,

which was one ninetieth part of an inch only. It is true, that HALLER is disposed to question the accuracy of this observation, and to attribute a much greater velocity to the blood flowing through the capillary vessels, but he did not attempt either to measure the velocity, or to determine it by calculation: nor is this the only instance in which HALLER has been led to reason erroneousiy, from a want of mathematical knowledge: he may, however, have observed the particles of blood moving in the axis of a vessel with a velocity much exceeding the mean velocity of its whole contents. If we calculate upon these foundations, from the formula which I have already laid before the Society, it will appear that the resistance which the friction of the arteries would occasion, if water circulated in them instead of blood, with an equal velocity, must amount to a force equivalent to the pressure of a column of fifteen inches and a half: to this we may add about a fourth for the resistance of the capillary veins, and we may estimate the whole friction for water, at twenty inches. The only considerable part of this force is derived from the term $\frac{2.1126/v}{10^3 d^{1.5}}$ in the value of f: this term increases for each successive segment in the ratio $1 : 1.49425 = 1 : n$, and the sum of the series is to the first term, as $\frac{n^{25}-1}{n-1}$ to 1. It appears also, that a very small portion only of the resistance is created in the larger vessels: thus, as far as the twentieth division, at the distance of an inch and a quarter only from the extreme capillary arteries, the pressure of a column of one twentieth of an inch only is required for overcoming the whole friction, and at the twenty fifth division, where the artery does not much exceed the diameter of a human hair, the height to which the

water would rise, in a tube fixed laterally into the artery, is only two inches less than in the immediate neighbourhood of the heart.

In order to judge of the comparative resistance produced by fluids of different degrees of viscidity, I employed the same tubes, by means of which I had determined the friction of water, in extreme cases, for ascertaining the effect of different substances held in solution in the water: since it is impossible to make direct experiments on the blood in its natural state, on account of its tendency to coagulate: and those substances which have the power of preventing its coagulation, may naturally be supposed to produce a material change in its viscidity. The diameter of one of the tubes, which was cylindrical, was the fortieth part of an inch: the bore of the other was oval, as is usual in the finest tubes made for thermometers: the section, divided by one fourth of the circumference, gave one hundred and seventy seconds for the mean diameter. I caused some milk, and solutions of sugar of different strength, to pass through these tubes: they were all transmitted much more sparingly than water, with an equal pressure, and the difference was more considerable in the smaller than in the larger tube, as might naturally be expected both from the nature of the resistance, and from the result of GERSTNER's experiments on water at different temperatures. In the first tube the resistance to the motion of milk was three times as great as to that of water, a solution of sugar in five times its weight of water produced twice as much resistance as water; in twice its weight, nearly four times as much as water: but in the narrower tube, the weaker solution of sugar exhibited a resistance five times as great as that of

water, which is more than twice as much as appeared in the larger tube. Hence there can be no doubt that the resistance of the internal surface of the arteries to the motion of the blood must be much greater than would be found in the case of water: and supposing it about four times as great, instead of 20 inches, we shall have 80, for the measure of a column of which the pressure is capable of forcing the blood, in its natural course, through the smaller arteries and veins, which agrees very well with HALES's estimate.

This determination of the probable dimensions of the arterial system, and of the resistances occasioned by its different parts, is in some few respects arbitrary, at the same time that it cannot be materially altered without altering either the whole quantity of blood contained in the body, the diameters of the smallest capillary vessels, the mean number of bifurcations, or the magnitude of the resistance, all of which are here assumed nearly as they have been laid down by former observers: the estimation of the length of the successive segments only is made in such a manner, as to reconcile these data with each other, by means of the experiments and calculations relating to the friction of fluids in pipes. The effect of curvature in increasing the resistance has been hitherto neglected; it can be only sensible in the larger vessels; and supposing the flexures of these to be equivalent to the circumferences of two circles, each two inches in diameter, the radius q being 1, we have $r = \frac{.0000045 \rho v^2 q^{\frac{1}{2}}}{q} = .0000045 \times 720 \times 64$ $= .207$, or about one fifth of an inch, for the additional resistance arising from this cause in the case of water, or four fifths for blood, which is a very inconsiderable part of the whole.

It might be questioned whether the experiments, which I have made, with tubes $\frac{1}{17}$ of an inch in diameter, are sufficient for determining, with accuracy, the degree in which the resistance would be increased in tubes, of which the diameter is only one sixth part as great; and it may be doubted whether the analogy, derived from these experiments, can be safely employed as a ground for asserting, that so large a portion of the arterial pressure is employed in overcoming the resistance of the very minute arteries. But it must be remembered, that these experiments are at least conclusive with respect to the arteries larger than the tubes employed in them, and even those which are a little smaller; so that the remaining pressure, as observed in experiments, can only be employed in overcoming the resistance of the minuter arteries and veins, and these observations tend therefore immediately to confirm the analogy drawn from the experiments on the motion of water. It might indeed be asserted, that the viscidity of the blood exceeds that of water in a much greater ratio than that which is here assigned; but this is rendered improbable by some experiments of Hales, in which, when the intestines were laid open, on the side opposite to the mesentery, so that many of the smaller arteries were divided, the quantity of warm water which passed through them with an equal pressure, was only about twelve times as great as that of the blood which flows through them in their natural state; and it is probable, that at least three or four times as much of any fluid must have passed through them in their divided, as in their entire state, unless we suppose that the coats of the divided vessels, like many other muscular parts, are capable of being contracted by the contact of water. In

some other experiments, it was found that a moderate degree of pressure was capable of causing water to exude so copiously through the exhalant vessels of the intestines, that it passed through the aorta with a velocity of about two inches in a second, although these vessels do not naturally allow any passage to the blood: on the other hand, it sometimes happened that very little water would pass through such channels as naturally transmitted a much larger quantity of blood: a circumstance which Dr. HALES very judiciously attributes to the oozing of the water into the cellular membrane surrounding the vessels, by means of which they were compressed, and their diameters lessened. On the whole, it is not improbable, that in some cases the resistance, opposed to the motion of the blood, may exceed that of water in a ratio somewhat greater than I have assigned; but this must be in the minutest of the vessels, while in the larger arteries the disproportion must be less: so that, however we may view the subject, it appears to be established, that the only considerable resistance which the blood experiences, occurs in the extreme capillary arteries, of which the diameter scarcely exceeds the hundredth part of an inch.

We cannot suppose that the dimensions of the sanguiferous system agree uniformly, in all its parts, with the measures which I have laid down; but the truth of the inference is not affected by these variations. For example, there may perhaps be some arteries communicating with veins, of which the diameter exceeds the eleven hundredth of an inch; but there are certainly many others which are much more minute; and the blood, or its more liquid parts, passing through these more slowly, it must move more rapidly in the former, so that the

resistance may in all be equal to the pressure, and the mean velocity may still remain such as is determined by the quantity of blood passing through the aorta. There is indeed some uncertainty in the measure of the globules of the blood, which I have made the basis of the dimensions of the minute arteries: and I have reason to think, that instead of $\frac{1}{2000}$ of an inch, their greatest diameter does not exceed $\frac{1}{3000}$, or even $\frac{1}{3600}$: the general results of the investigation are not however affected by this difference: it will only require us to suppose the subdivisions somewhat more numerous, and the branches shorter.

These are the principal circumstances which require to be considered, with respect to the simple transmission of the blood through the arteries into the veins, without regard to the alternate motions of the heart, and to the elastic and muscular powers of the vessels. I shall next examine the nature and velocity of the propagation of the pulse. The successive transmission of the pulsations of the heart, through the length of the arteries, is so analogous to the motion of the waves on the surface of water, or to that of a sound transmitted through the air, that the same calculations will serve for determining the principal affections of all these kinds of motion; and if the water, which is agitated by waves, is supposed to flow at the same time in a continued stream, and the air which conveys a sound to be carried forwards also in the form of a wind, the similitude will be still stronger. The coats of the arteries may perhaps be considered, without much inaccuracy, as perfectly elastic, that is, as producing a force proportional to the degree in which they are extended beyond their natural dimensions; but it is not impossible that there may be some bodies in nature, which differ materially from this general

law, especially where the distension becomes considerable: thus there may be substances which exhibit a force of tension proportional to the excess of the square, or the cube of their length, beyond a certain given quantity. It is safest therefore to reason upon the elasticity of any substance, from experiments made without any great deviation from the circumstances to which the calculation is to be applied.

For this purpose, we may again employ some of the many excellent experiments contained in HALES's hæmastatics. It appears, that when any small alteration was made in the quantity of blood contained in the arteries of an animal, the height of the column, which measured the pressure, was altered nearly in the same proportion, as far as we are capable of estimating the quantity, which was probably contained in the larger vessels of the animal. Hence it follows, that the velocity of the pulse must be nearly the same as that of an impulse transmitted through an elastic fluid, under the pressure of a column of the same height, as that which measures the actual arterial pressure: that is, equal to that which is acquired by a heavy body falling freely through half this height. In man, this velocity becomes about fifteen feet and a half in a second; to which the progressive motion of the blood itself adds about eight inches; and with this velocity, of at least sixteen feet in a second, it may easily happen that the pulse may appear to arrive at the most distant parts of the body without the intervention of any very perceptible interval of time.

The velocity of the transmission of the pulse being known, it is easy to determine the degree in which the arteries are dilated during its passage through them. The mean velocity of the blood in the aorta being eight inches and a half in a

second, its greatest velocity must be about three times as much, since the contraction of the heart is supposed to occupy only about one third part of the interval between two successive pulsations; and if the velocity of the pulse is sixteen feet in a second, that of the blood itself must be about one eighth part as great; so that the column of blood occupying eight inches may occupy only seven; hence the diameter must increase in the ratio of about fifteen to sixteen. The tension will also become one eighth greater, and the force of the heart must be capable of supporting a column of one hundred and one inches. This force would, however, require to be somewhat increased, from the consideration that the force required at the end of any canal during the reflection of a pulsation or wave of any kind, is twice as great as the force exerted during its transmission, and the force employed in the origination of a wave or pulse in a quiescent fluid, is the same as is required for its reflection; on the other hand, a weaker pulsation, proceeding into a narrower channel, becomes more energetic, so that, from this consideration, a force somewhat smaller would be required in the heart: on the whole, however, it appears probable, that the former of these corrections must be the more considerable, and that the force of the heart must be measured by the pressure of a column, rather more than less than one hundred and one inches high: nor would this force by any means require a strong exertion of muscular power; for it only implies a tension of something less than three pounds for each inch of the circumference of the greatest section of the heart; and supposing the mean thickness half an inch, an equal number of the fibres of some other muscles of the body would be capable of exerting a force of

more than two hundred pounds, in the state of the greatest possible action.

The force, here assigned to each pulsation, agrees extremely well with the inference that may be drawn from an experiment of HALES, on the ascent of the blood in a tube connected with an artery of a horse. The whole height of the column being nine feet, the blood rose about three inches higher during each pulsation, which was repeated fifty or sixty times in a minute: now we may suppose the acceleration to have extended a little beyond the first half of the space thus described, so that two inches were described in two fifths of a second; and if there had been no friction, nor any other cause of retardation, there can be no doubt that at least four inches would have been described in the same time; but the same column of nine feet, if it had been actuated by its own weight, would have described thirty one inches in the same time: consequently the force with which the blood was forced through the artery was nearly one eighth of the whole force of tension, as it appears in the former calculation.

The magnitude of the pulse must diminish in the smaller arteries in the subduplicate proportion of the increase of the joint areas, in the same manner as the intensity of sound is shewn to decrease in diverging from a centre, in the subduplicate ratio of the quantity of matter affected by its motion at the same time. For example, in the arteries of the tenth order, of which the diameter is one thirteenth of an inch, its magnitude must be only one third as great as in the aorta, that is, the greatest progressive velocity of the blood must be eight inches and a half in a second only, and the dilatation one fiftieth part only of the diameter. In the vessels of the

twentieth order, the dilatation does not exceed $\frac{1}{160}$ of the diameter, which is itself the 140th part only of an inch: so that it is not surprising, that HALLER should have been unable to discover any dilatation in vessels of these dimensions, even with the assistance of a powerful microscope. If we estimated the magnitude of the pulse in the aorta, from the excess of the temporary above the mean velocity, which would perhaps be justifiable, that magnitude would become still less considerable.

These calculations agree extremely well with each other, and with experiment, as far as they relate to the power of the heart, and the affections of the smaller arteries. But there is reason to think that the velocity of the pulse in the larger vessels is much more considerable than has been here stated; and their dilatation is also less conspicuous, when they are exposed to view, than it would probably be, if it were as great as is inferred from the velocity here assigned. I have demonstrated, in the hydraulic investigations which I lately laid before the Royal Society, that the velocity of an impulse passing through a tube, consisting of perfectly elastic materials, is half as great as that of a body supposed to have fallen from the given point to the base of the modular column of the tube: and that the height of this column is such that the tube would be extended without limit by its pressure; consequently it must be greater than the height of a column equivalent to the pressure by which the tube is burst. Now it has been ascertained by Dr. HALES, that the pressure, required for bursting one of the carotids of a dog, is equal to that of a column of water one hundred and ninety feet high; nor does he remark that the artery was very materially dilated; and deducting from this height the five feet which express the

actual pressure in the arteries of a dog, the remaining one hundred and eighty five feet will give a velocity of at least fifty four feet in a second, for the propagation of the pulse in the dog. It is not however ascertained, that all the membranes, which may have surrounded the artery in this experiment, are called into action in its ordinary pulsation, much less that the force, developed by their tension, varies precisely according to the general law of perfectly elastic bodies: but this mode of calculation is still amply sufficient to make it probable, that the velocity of the pulsations, in the larger arteries, must amount to at least forty feet in a second, although some very considerable deductions must be made, on account of the resistances of various kinds, which cannot be comprehended in the calculation.

The artery must not be supposed to subside, immediately after each pulsation, precisely to its original dimensions, since it must remain somewhat fuller, in order to supply the capillary arteries, and the veins, in the interval between the two successive pulsations: and in this respect it differs from the motions of a wave through a canal, which is open on both sides: but the difference may be understood, by supposing a partial reflection of the pulse to take place at every point where it meets with any resistance, which will leave a general distension of the artery, without any appearance of a retrograde pulsation.

I shall proceed to inquire, in the third place, into the nature and extent of the functions which are to be attributed to the muscular fibres of the coats of the arteries; and I apprehend that it will appear to be demonstrable, that they are much less concerned in the progressive motion of the blood, than

is almost universally believed. The arguments, which may be employed to prove this, are nearly the same that I have already stated, in examining the motion of a fluid, carried along before a moving body in an open canal; but in the case of an elastic tube, the velocity of the transmission of an impulse being rather diminished than increased by an increase of tension, the reasoning is still stronger and simpler; for it may here be safely asserted, that the anterior parts of the dilatation, which must be forced along by any progressive contraction of the tube, can only advance with the velocity appropriate to the tube, and that its capacity must be proportionate to its length and to the area of its section: now the magnitude of its section must be limited by that degree of tension which is sufficient to force back through the contraction what remains of the displaced fluid, and the length by the difference of the velocity appropriate to the tube, and that with which the contraction advances; consequently if the contraction advance with the velocity of a pulsation, as any contractile action of the arteries must be supposed to do, this length necessarily vanishes, and with it the quantity of the fluid protruded; the whole being forced backwards, by the distending force which is exerted by a very small dilated portion, immediately preceding the contraction. It might indeed be imagined, that the contraction follows the pulsation with a velocity somewhat smaller than its own; but this opinion would stand on no other foundation than mere conjecture, and it would follow, that the pulse would always become more and more full, as it became more distant from the heart; of which we have nothing like evidence: nor would a moderate contraction, even if this supposition were granted, produce any material effect. For

example, if the velocity of the contraction were only half as great as that of the pulsation, which is the most favourable proportion, it would be necessary, taking sixteen feet in a second for the velocity of the pulsation, that the section of the arteries should be contracted to about one half, in order to produce, by their progressive contraction only, the actual velocity of the blood in the aorta; one sixteenth of the blood being carried, in this case, before the contraction: but if the contraction were only such, as to reduce the section of the artery to $\frac{9}{10}$, which is probably more than ever actually happens, the velocity produced would be only about $\frac{1}{10}$ as much; and if the contraction were only to $\frac{99}{100}$, which is a sufficient allowance for the smaller arteries, about $\frac{1}{10000}$ only of the actual velocity in the aorta could be produced in this manner, even upon a supposition much more favourable to the muscular action of the arteries than the actual circumstances. A small addition must be made to the force required for producing the retrograde motion, on account of the friction to be overcome, but the general reasoning is not affected by this correction.

The contraction of the artery might also be supposed to remain after each pulsation, so that the vessel should not be again dilated until the next pulsation, or, in other words, a spontaneous dilatation might be supposed to accompany the pulsation, instead of a contraction: but such a dilatation would be useless in promoting the progressive motion of the blood, since a larger quantity of blood, conveyed to the smaller vessels, without an increased tension, would be ineffectual with respect to the resistances which are to be overcome. It is possible indeed that the muscular fibres of those arteries in which the magnitude of the pulse is sensible, like the fibres of

the heart, may be inactive, or nearly so, during their dilatation, and that they may contract after they have been once distended, with a force which is in a certain degree permanent; the greater momentum of the blood, which accompanies the dilatation, enabling it to enter the minute arteries with equal ease, although assisted by a tension somewhat smaller: so that the same mean velocity may be sustained, as if the arteries were simply elastic, and a little smaller in diameter, with a very little less exertion of the heart. But the distribution of the blood could never be materially diversified by any operation of this kind: for if any artery were for a moment distended by such a variation, so as to exceed its natural diameter by one hundredth part only, a pressure would thence arise equivalent to that of a column about two inches high, which would, in spite of all resistances, immediately dissipate the blood with a considerable velocity, and completely prevent any local accumulation, unless the elastic powers of the vessel itself were diminished; and this is, perhaps, the most important, as well as the best established inference from the doctrine that I have advanced.

It appears that a mola has sometimes been found in the uterus, totally destitute of a heart, in which the blood must have circulated in its usual course through the veins and arteries: in this case it cannot be ascertained whether there was any alternate pulsation, or whether the blood was carried on in a uniform current, in the same manner as the sap of a vegetable probably circulates. If there was a pulsation, it may have been maintained by a contraction of the artery, much more considerable, and slower in its progress than usual; and with the assistance of a spontaneous dilatation; the resistance

in the extreme vessels being also probably much smaller than usual: if the motion was continued, it would lead us to imagine that there may be some structure in the placenta capable of assisting in the propulsion of the blood, as there may possibly be some arrangement in the roots of plants by which they are calculated to promote the ascent of the sap. The circulation in the vessels of the more imperfect animals, in which a great artery supplies the place of a heart, is of a very different nature from that of the more perfect animals: the great artery, which performs the office of the heart, is here possessed of a muscular power commensurate to its functions, and seems to propel the blood, though much more slowly than in other cases, by means of a true peristaltic motion. It appears also from the observations of SPALLANZANI, that in many animals a portion of the aorta, next the heart, is capable of exhibiting a continued pulsation, even when perfectly empty and separated from the heart; but this property is limited to a small part of the artery only, which is obviously capable of being essentially useful in propelling the blood when the valves of the aorta are closed. The muscular power of the termination of the vena cava is also capable of assisting the passage of the blood into the auricle. It is not at all improbable that a muscle of involuntary motion, which had been affected throughout the whole period of life by alternate contractions and relaxations, might retain from habit the tendency to such contractions, even without the necessity of supposing, that the habit was originally formed for a purpose to be obtained by the immediate exertion of the muscular power: but in fact the partial pulsation of the vena cava is perfectly well calculated to promote the temporary repletion of the

auricle, while it must retard, for a moment, the column which is approaching, at a time that it could not be received.

There is no difficulty in imagining what services the muscular coats of the arteries may be capable of performing, without attributing to them any immediate concern in supporting the circulation. For since the quantity of blood in the system is on many accounts perpetually varying, there must be some means of accommodating the blood vessels to their contents. This circumstance was very evident in some of HALES's experiments, when after a certain quantity of blood had been taken away, the height of the column, which measured the tension of the vessels, frequently varied in an irregular manner, before it became stationary at a height proportional to the remaining permanent tension. HALLER also relates, that he has frequently seen the arteries completely empty, although in some of his observations there was probably only a want of red globules in the blood which was flowing through them. Such alterations in the capacity of the different parts of the body are almost always to be attributed to the exertion of a muscular power. A partial contraction of the coats of the smaller arteries may also have an immediate effect on the quantity of blood contained in any part, although very little variation could be produced in this manner by a change of the capacity of the larger vessels.

According to this statement of the powers which are concerned in the circulation, it must be obvious that the nature of the pulse, as perceptible to the touch, must depend almost entirely on the action of the heart, since the state of the arteries can produce very little alteration in its qualities. The greater or less tension of the arterial system may indeed render

the artery itself, when at rest, somewhat harder or softer; and, if the longitudinal fibres give way to the tending force, it may become also tortuous: possibly too a very delicate touch may in some cases perceive a difference in the degree of dilatation, although it is seldom practicable to distinguish the artery, in its quiescent state, from the surrounding parts. But the sensation, which is perceived when the artery is compressed, as usual, by the finger, is by no means to be confounded with the dilatation of the artery; for in this case an obstacle is opposed to the motion of the blood, against which it strikes, with the momentum of a considerable column, almost in the same manner as a stream of water strikes on the valve of the hydraulic ram; and in this manner, neglecting the difference of force arising from the different magnitudes of the sections, the pressure felt by the finger becomes nearly equal and similar to that which is originally exerted by the heart: each pulsation passing under the finger, in the same time, as is required for the contraction of the heart, although a very little later; and more or less so, in proportion as the artery is more or less distant; the artery remaining then at rest for a time equal to that in which the heart is at rest. When therefore an artery appears to throb, or to beat more strongly than usual, the circumstance is only to be explained from its greater dilatation, which allows it to receive a greater portion of the action of the heart, in the same manner as an aneurysm exhibits a very strong pulsation, without any increase of energy, either in itself, or in the neighbouring vessels; and on the other hand, when the pulsations of the artery of a paralytic arm become feeble, we cannot hesitate to attribute the change to its permanent con-

traction, since the enlargement and contraction of the blood-vessels of a limb are well known to attend the increase or diminution of its muscular exertions. There is also another way, in which the diminution of the strength of an artery may increase the apparent magnitude of the pulse, that is, by diminishing the velocity with which the pulsation is transmitted: for we have seen that the magnitude of the pulse is in the inverse ratio of the length of the artery distended at once; and this length is proportional to the velocity of the transmission: but it must be observed, that the force of the pulse striking the finger would not be affected by such a change, except that it might be rendered somewhat fuller and softer, although a considerable throbbing might be felt in the part, from the increased distension of the temporary diameter of the artery. How little a muscular force is necessary for the simple transmission of a pulsation, may easily be shown by placing a finger on the vena saphena, and striking it with the other hand at a distant part; a sensation will then be felt precisely like that of a weak arterial pulsation.

The deviations from the natural state of the circulation, which are now to be cursorily investigated, may be either general or partial; and the general deviations may consist either in a change of the motion of the heart, or of the capacity of the capillary arteries. When the motion of the heart is affected, the quantity of blood transmitted by it may either remain the same as in perfect health, or be diminished, or increased. Supposing it to remain the same, the pulse, if more frequent, must be weaker, and if slower, it must be stronger; but this latter combination is scarcely ever observable; and in the former case, the heart must either never be filled, perhaps

on account of too great irritability, or never be emptied, from the weakness of its muscular powers. But the immediate effect of such a change as this, in the functions depending on the circulation, cannot be very material, and it can only be considered as an indication of a derangement in the nervous and muscular system, which is not likely to lead to any disease of the vital functions. When the quantity of the blood transmitted by the heart is smaller than in health, the arteries must be contracted, until their tension becomes only adequate to propel the blood, through the capillary vessels, with a proportionally smaller velocity, and the veins must of course become distended, unless the muscular coats of the arteries can be sufficiently relaxed to afford a diminished tension, which is probably possible in a very limited degree only. In this state the pulse must be small and weak, and the arteries being partly exhausted, there will probably be a paleness and chilliness of the extremities: until the blood, which is accumulated in the veins, has sufficient power to urge the heart to a greater action, and perhaps, from the vigour which it may have acquired during the remission of its exertions, even to a morbid excess of activity. Hence a contrary state may arise, in which the quantity of blood transmitted by the heart is greater than in perfect health; the pulse will then be full and strong, the arteries being distended, so as to be capable of exerting a pressure sufficient to maintain an increased velocity, and to overcome the consequent increase of resistance; a state which perhaps constitutes the hot fit of fever; and which is probably sometimes removed in consequence of a relaxation of the extreme arteries, which suffer the superfluous blood to pass more easily into the veins. Such a relaxation, when carried

to a morbid extent, may also be a principal cause of another general derangement of the circulation, the motion of the blood being accelerated, and the arteries emptied, so that the pulse may be small and weak, while the veins are overcharged, and the heart exhausted by violent and fruitless efforts to restore the equilibrium; and this state appears to resemble, in many respects, the affections observed in typhus. When, on the contrary, the capillary vessels are contracted, the arteries are again distended, although without the excess of heat which must attend their distension from an increased action of the heart, and possibly without fever: an instance of this appears to be exhibited in the shrinking of the skin, which is frequently observable from the effect of cold, and in the first impression produced by a cold bath: nor is it impossible, that such a contraction may exist in the cold fit of an intermittent, although it seems more probable that a debility of the heart is the primary cause of this affection.

Besides these general causes of derangement, which appear to be more or less concerned in different kinds of fever, there are other more partial ones, which seem to have a similar relation to local inflammations. The most obvious of these changes are such as must be produced by partial dilatations or contractions of the capillary vessels; since, as I have endeavoured to demonstrate, any supposed derangement in the actions of the larger vessels must be excluded from the number of causes which can materially affect the circulation. It cannot be denied, that a diminution of the elastic, or even of the muscular force of the small arteries, must be immediately followed by such a distension as will produce a resistance equal to the pressure: the distension will occasion an increase

of redness, and in most cases pain: the heat will also generally be increased, on account of the increased quantity of blood which will be allowed to pass through the part; and since the hydrostatic pressure of the blood acquires greater force, as the artery becomes more distended, it may be so weak as to continue to give way, like a ligament which has been strained, until supported by the surrounding parts. In this state a larger supply of blood will be ready for any purposes which require it, whether an injury is to be repaired, or a new substance formed; and it is not impossible, that this change in the state of the minute vessels may ultimately produce some change in the properties of the blood itself.

The more the capillary arteries are debilitated and distended, the greater will be the mean velocity of the circulation; but whether or no the velocity will be increased in the vessels which are thus distended, must depend on the extent of the affected part; and it may frequently happen that the velocity may be much more diminished on account of the dilatation of the space which the blood is to occupy, than increased by the diminution of the resistance. And on the other hand, the velocity may be often increased, for a similar reason, at the place of a partial contraction. Hence we may easily understand some of the experiments which Dr. Wilson has related in his valuable treatise on fevers: the application of spirit of wine to a part of the membrane of a frog's foot contracted the capillary arteries, and at the same time accelerated the motion of the blood in them, while in other parts, where inflammation was present, and the vessels were distended, the motion of the blood was slower than usual.

Another species of inflammation may probably be occa-

sioned by a partial constriction or obstruction of the capillary arteries, which must indeed be supposed to exist where the blood has become wholly stagnant, as Dr. WILSON in some instances found it. This obstruction must however be extended to almost all the branches, belonging to some small trunk, in which the pressure remains nearly equal to the tension of the large arteries: for in this case it will happen, that the whole pressure will be continued throughout the obstructed branches, without the subtraction of the most considerable part, which is usually expended in overcoming the resistances dependent on the velocity; so that the small branches will be subjected to a pressure, many times greater than that which they are intended to withstand in the natural state of the circulation; whence it may easily happen that they may be morbidly distended; and this distension may constitute an inflammation, attended by redness and pain. Nor is it impossible, that obstructions of this kind may originate in a vitiated state of the blood itself, although it would be difficult to prove the truth of the conjecture; it seems, however, to be favoured by the observation of HALLER, that little clots of globules may often be observed in the arteries, when the circulation is languid, and that they disappear when its vigour is restored, especially after venesection. But if a very small number only of capillary arteries be obstructed, other minute branches will still be capable of receiving the blood, which ought to pass through them, without any great distension or increase of pressure: and this exception is sufficient to explain another experiment of Dr. WILSON, in which a small obstruction, caused by puncturing a membrane with a hot needle, failed to excite an inflammation. This species of inflammation is probably

attended by less heat than the former; and where the obstruction is very great, it may perhaps lead immediately to a mortification, which is called by the Germans "a cold burning."

The most usual causes of inflammation appear to be easily reconcileable with these conjectures. Suppose any considerable part of the body to be affected by cold; the capillary vessels will be contracted, and at the same time the temperature of some parts of their contents will be lowered, from both of which causes the resistance will be increased, and the arteries in general will be more or less overcharged: if then any other part of the system be at the same time debilitated or overheated, its arteries will be liable to be morbidly distended, and an inflammation may thus arise, which may continue till the minute vessels are supported and strengthened, by means of an effusion of coagulable lymph. The immediate effect, either of cold or of heat, may also sometimes produce such a degree of debility in any part, as may lay the foundation of a subsequent inflammation: but the first effect of heat in the blood-vessels appears to be the more ready transmission of the blood into the veins, by means of which they become very observably prominent: and cold, which checks the circulation in the cutaneous vessels, probably occasions a livid hue, by retaining the blood stagnant longer than usual in the capillary vessels of all kinds. It may be objected, that an obstruction of the motion of the blood through a great artery ought, upon these principles, to produce an inflammation in some distant part: but in this case, the blood will still find its way very copiously into the parts supplied by the artery, by means of some collateral branches, which will always admit a much larger quantity of blood than usually passes through them, whenever

a very slight excess of force can be found to carry it on, or when the blood which they contain finds a readier passage than usual, by means of their communication with such parts as are now deprived of their natural supply.

It is difficult to determine, whether blushing is more probably effected by a constriction or by a relaxation of the vessels concerned; it must, however, be chiefly an affection of the smaller vessels, since the larger ones do not contain a sufficient quantity of blood to produce so sudden an effect. Perhaps the capillary vessels are dilated, while the arteries, which are a little larger only, are contracted: possibly too an obstruction may exist at the point of junction of the arteries with the veins; and where the blush is preceded by paleness, such an obstruction is probably the principal cause of the whole affection.

With respect to the tendency of inflammation in general to extend itself to the neighbouring parts, it is scarcely possible to form any reasonable conjecture that can lead to its explanation: this circumstance appears to be placed beyond the reach of any mechanical theory, and to belong rather to some mutual communication of the functions of the nervous system, since it is not inflammation only, that is thus propagated, but a variety of other local affections of a specific nature, which are usually complicated with inflammation, although they may perhaps, in some cases, be independent of it. Inflammations, however, are certainly capable of great diversity in their nature, and it is not to be expected, that any mechanical theory can do more than to afford a probable explanation of the most material circumstances, which are common to all the different species.

Besides these general illustrations of the nature of fevers and inflammations, the theory which has been explained may sometimes be of use, in enabling us to understand the operation of the remedies employed for relieving them. Thus it may be shown, that any diminution of the tension of the arterial system must be propagated from the point at which it begins, as from a centre, nearly in the same manner, and with the same velocity, as an increase of tension, or a pulsation of any kind would be propagated. Hence the effect of venesection must be not only more rapidly, but also more powerfully felt in a neighbouring than in a distant part: and although the mean or permanent tension of the vessels of any part must be the same, from whatever vein the blood may have been drawn, provided that they undergo no local alteration, yet the temporary change, produced by opening a vein in their neighbouhood, may have relieved them so effectually from an excess of pressure, as to allow them to recover their natural tone, which they could not have done without such a partial exhaustion of the neighbouring vessels. But since it seems probable, that the minute arteries are more affected by distension than the veins, there is reason in general to expect a more speedy and efficacious relief in inflammations, from opening an artery than a vein: this operation, however, can seldom be performed without material inconvenience; but it is probably for a similar reason, that greater benefit is often experienced from withdrawing a small portion of blood by means of cupping or of leeches, than a much larger quantity by venesection, since both the former modes of bleeding tend to relieve the arteries, as immediately as the veins, from that distension, which appears to constitute the most essential characteristic of

inflammation. In a case of hemorrhage from one of the sinuses of the brain, a very judicious physician lately prescribed the digitalis: if the effect of this medicine tends principally to diminish the action of the heart, as is commonly supposed, it was more likely to be injurious than beneficial, since a venous plethora must be increased by the inactivity of the heart; but if the digitalis diminishes the general tension of the arteries, in a greater proportion than it affects the motion of the heart, it may possibly be advantageous in venous hemorrhages. We have, however, no sufficient authority for believing, that it has any such effect on the arterial system in general.

Although the arguments, which I have advanced, appear to me sufficient to prove, that, in the ordinary state of the circulation, the muscular powers of the arteries have very little effect in propelling the blood, yet I neither expect nor desire that the prevailing opinion should at once be universally abandoned. I wish, however, to protest once more against a hasty rejection of my theory, from a superficial consideration of cases, like that which has been related by Dr. CLARKE; and to observe again, that the objections, which I have adduced, against the operation of the muscular powers of the arteries in the ordinary circulation, not being applicable to these cases, they are by no means weakened by any inferences which can be drawn from them.

ERRATA.

In the last volume of the Philosophical Transactions, page 183, line 25, for $\frac{4}{1}$, read $\frac{a}{3}$: page 184, at the end, add, is denoted by *m*: page 186, line 4, for when *end*, read whence *d*.

II. *An Account of some Experiments, performed with a View to ascertain the most advantageous Method of constructing a Voltaic Apparatus, for the Purposes of Chemical Research.* By John George Children, *Esq. F. R. S.*

Read November 24, 1808.

THE late interesting discoveries by Mr. DAVY, having shewn the high importance of the VOLTAIC battery, as an instrument of chemical analysis, it became a desirable object to ascertain that mode of constructing it, by which the greatest effect may be produced, with the least waste of power and expense.

For this purpose, I made a battery, on the new method, with plates of copper and zinc, connected together by leaden straps, soldered on the top of each pair of plates; which are twenty in number, and each plate four feet high, by two feet wide: the sum of all the surfaces being 92160 square inches, exclusive of the single plate at each end of the battery. The trough is made of wood, with wooden partitions well covered with cement, to render them perfectly tight, so that no water can flow from one cell to another. The battery was charged with a mixture of three parts fuming nitrous, and one part sulphuric acid, diluted with thirty parts of water, and the quantity used was 120 gallons.

In the presence, and with the kind assistance of Messrs. DAVY, ALLEN, and PEPYS, the following experiments were made.

Experiment 1. Eighteen inches of platina wire, of $\frac{1}{30}$th of an inch diameter, were completely fused in about twenty seconds.

Exp. 2. Three feet of the same wire were heated to a bright red, visible by strong day-light.

Exp. 3. Four feet of the same wire were rendered very hot; but not perceptibly red by day-light. In the dark, it would probably have appeared red throughout.

Exp. 4. Charcoal burnt with intense brilliancy.

Exp. 5. On iron wire, of about $\frac{1}{70}$th of an inch diameter, the effect was strikingly feeble. It barely fused ten inches, and had not power to ignite three feet.

Exp. 6. Imperfect conductors were next submitted to the action of the battery, and barytes, mixed with the red oxyde of mercury, and made into a paste with pipe-clay and water, was placed in the circuit; but neither on this, nor on any other similar substance was the slightest effect produced.

Exp. 7. The gold leaves of the electrometer were not affected.

Exp. 8. When the cuticle was dry, no shock was given by this battery, and even though the skin was wet, it was scarcely perceptible.

Before I offer any observations on the inferences to be drawn from these experiments, I shall mention some others, performed, for the sake of comparison, with the foregoing, with an apparatus very different in size and number of plates, from the one just described.

This second battery was precisely the *Couronne des Tasses* of Sig. VOLTA, consisting of two hundred pairs of plates, each about two inches square, placed in half pint pots of common

queen's ware, and made active by some of the liquor used in exciting the large battery, to which was added a fresh portion of sulphuric acid, equal to about a quarter of a pint to a gallon.

To state as shortly as possible the effects produced by this battery:

Experiment 1. It decomposed potash and barytes readily.

Exp. 2. It produced the metallization of ammonia with great facility.

Exp. 3. It ignited charcoal vividly.

Exp. 4. It caused considerable divergence of the gold leaves of the electrometer.

Exp. 5. It gave a vivid spark, after being in action three hours. At the expiration of twenty-four hours, it retained sufficient power to metallize ammonia, and continued, with gradually decreasing energy, to produce the same effect, till the end of forty-one hours, when it seemed *nearly* exhausted.

From the results of the foregoing experiments, which though simple and not numerous, I trust, are satisfactory; we see Mr. DAVY'S theory of the mode of action of the VOLTAIC battery confirmed: he says (in his Paper on some Chemical Agencies of Electricity, Sect. 9. after having shewn the effect of induction to increase the electricity of the opposite plates), "the *intensity* increases with the *number*, and the *quantity* with the *extent* of the series."

That this is so, the effects produced on the platina and iron wires, in the first and fifth experiments with the large battery, and the subsequent experiments on imperfect conductors, with the small apparatus, sufficiently prove. The platina wire being a perfect conductor, and not liable to be oxydated, presents no obstacle to the free passage of the electricities through it,

which, from the immense quantities given out from so large a surface, evolve, on their mutual annihilation, heat sufficient to raise the temperature of the platina to the point of fusion.

With the iron wire, of $\frac{1}{70}$th of an inch diameter, the effect is very different, which is explained by the low state of the intensity of the electricity (sufficiently proved by its not causing any divergence of the gold leaves of the electrometer), which being opposed in its passage by the thin coat of oxide, formed on the iron wire, at the moment the circuit is completed, a very small portion only of it is transmitted through the wire. To the same want of intensity is to be attributed the total inability of the large battery to decompose the barytes, and its general weak action on bodies which are not perfect conductors. The small battery, on the contrary, exerts great power on imperfect conductors, decomposing them readily, although its whole surface is more than thirty times less than that of the great battery; but in point of number of plates, it consists of nearly ten times as many as the large one. The long continued action of the small battery, proves the utility of having the cells of sufficient capacity to hold a large quantity of liquor, by which much trouble of emptying and filling the troughs is avoided, and the action kept up, without intermission, for a long space of time, a circumstance, in many experiments, of material consequence. Besides this advantage, *with very large combinations*, a certain distance between each pair of plates is *absolutely necessary*, to prevent spontaneous discharges, which will otherwise ensue, accompanied with vivid flashes of electric light, as I have experienced, with a battery of 1250 four-inch plates, on the new construction. And here I beg leave to mention an experiment, which, though not

directly in point, cannot be considered as foreign to the subject of this Paper. It has been urged, as one proof of the non-identity of the common electricity, and that given out by the VOLTAIC apparatus, that in the latter there is no striking distance. That objection, however, must cease. I took a small receiver, open at one end; through perforations in the opposite sides of which were placed two wires, with platina points, well polished: one was fixed by cement to the glass, the other was moveable, by means of a fine screw, through a collar of leathers, and the distance between the points was ascertained by a small micrometer attached. This receiver was inverted over well dried potash over mercury, and suffered to stand a couple of days, to deprive the air it contained, as thoroughly as possible, of moisture. The 1250 plates being excited precisely to the same degree as the great battery, mentioned in the beginning of this communication; and the little receiver placed in the circuit, I ascertained its striking distance to be $\frac{1}{50}$th of an inch. That I might be certain that the air in the apparatus had not become a conductor by increase of temperature, I repeated the experiment several times with fresh cool air, and always with the same result; but perhaps it will be objected, that the striking distance was so small, as not to afford a satisfactory refutation of the argument alluded to, when it is considered to how very great a distance, comparatively, the spark of the common electrical machine can pass through air. The answer to this is obvious: increase the number of the plates, and the striking distance will increase; for we see throughout, the intensity proportioned to the number, and it probably may be carried to such extent, as even to pass through a thicker plate of air, than the common spark.

The great similarity of the appearance of the electric light of this battery in vacuo, and that of the common machine, might also be urged as an additional proof of the identity of their nature.

The effect of this large combination on imperfect conductors, was, as may be supposed, very great; but of the same platina wire, of which the four-feet plates fused eighteen inches, this battery melted but half an inch, though, had the effect been in the ratio of their surfaces, it should have fused nearly fourteen inches.

The absolute effect of a VOLTAIC apparatus, therefore, seems to be in the compound ratio of the number and size of the plates: the intensity of the electricity being as the former, the quantity given out as the latter; consequently regard must be had, in its construction, to the purposes for which it is designed. For experiments on perfect conductors, very large plates are to be preferred, a small number of which will probably be sufficient; but where the resistance of imperfect conductors is to be overcome, the combination must be great, but the size of the plates may be small; but if quantity and intensity be both required, then a large number of large plates will be necessary. For general purposes, four inches square will be found to be the most convenient size.

Of the two methods usually employed, that of having the copper and zinc plates joined together only in one point, and moveable, is much better than the old plan of soldering them together, through the whole surface, and cementing them into the troughs: as, by the new construction, the apparatus can be more easily cleaned and repaired, and a double quantity of surface is obtained. For the partitions in the troughs,

glass seems the substance best adapted to secure a perfect insulation; but the best of all, will be troughs made entirely of WEDGWOOD's ware, an idea, I believe, first suggested by Dr. BABINGTON.

III. *The Bakerian Lecture. An Account of some new analytical Researches on the Nature of certain Bodies, particularly the Alkalies, Phosphorus, Sulphur, Carbonaceous Matter, and the Acids hitherto undecompounded; with some general Observations on Chemical Theory.* By Humphry Davy, *Esq. Sec. R. S. F. R. S. Ed. and M. R. I. A.*

Read December 15, 1808.

1. *Introduction.*

In the following pages, I shall do myself the honour of laying before the Royal Society, an account of the results of the different experiments, made with the hopes of extending our knowledge of the principles of bodies by the new powers and methods arising from the applications of electricity to chemistry, some of which have been long in progress, and others of which have been instituted since their last session.

The objects which have principally occupied my attention, are the elementary matter of ammonia, the nature of phosphorus, sulphur, charcoal, and the diamond, and the constituents of the boracic, fluoric, and muriatic acids.

Amongst the numerous processes of decomposition, which I have attempted, many have been successful; and from those which have failed, some new phenomena have usually resulted which may possibly serve as guides in future inquiries. On this account, I shall keep back no part of the investigation, and I shall trust to the candour of the Society for an excuse for its imperfection.

The more approaches are made in chemical inquiries towards the refined analysis of bodies, the greater are the obstacles which present themselves, and the less perfect the results.

All the difficulties which occur in analysing a body, are direct proofs of the energy of attraction of its constituent parts. In the play of affinities with respect to secondary compounds even, it rarely occurs that any perfectly pure or unmixed substance is obtained; and the principle applies still more strongly to primary combinations.

The first methods of experimenting on new objects likewise are necessarily imperfect; novel instruments are demanded, the use of which is only gradually acquired, and a number of experiments of the same kind must be made, before one is obtained from which correct data for conclusions can be drawn.

2. *Experiments on the Action of Potassium on Ammonia, and Observations on the Nature of these two Bodies.*

In the Bakerian lecture, which I had the honour of reading before the Society, November 19, 1807, I mentioned that in heating potassium strongly in ammonia, I found that there was a considerable increase of volume of the gas, that hydrogene and nitrogene were produced, and that the potassium appeared to be oxidated; but this experiment, as I had not been able to examine the residuum with accuracy, I did not publish. I stated it as an evidence, which I intended to pursue more fully, of the existence of oxygene in ammonia.

In a paper read before the Royal Society last June, which they have done me the honour of printing, I have given an

account of various experiments on the amalgam from ammonia, discovered by Messrs. BERZELIUS and PONTIN, and in a note attached to this communication, I ventured to controvert an opinion of M. M. GAY LUSSAC and THENARD, with respect to the agency of potassium and ammonia, even on their own statement of facts, as detailed in the Moniteur for May 27, 1808.

The general obscurity belonging to these refined objects of research, their importance and connection with the whole of chemical theory, have induced me since that time to apply to them no inconsiderable degree of labour and attention; and the results of my inquiries will, I trust, be found not only to confirm my former conclusions; but likewise to offer some novel views.

In the first of these series of operations on the action of potassium on ammonia, I used retorts of green glass; I then suspecting oxygene might be derived from the metallic oxides in the green glass, employed retorts of plate glass, and last of all, I fastened the potassium upon trays of platina, or iron, which were introduced into the glass retorts furnished with stop cocks. These retorts were exhausted by an excellent air pump, they were filled with hydrogene, exhausted a second time, and then filled with ammonia from an appropriate mercurial gas holder.* In this way the gas was operated upon in a high degree of purity, which was always ascertained; and all the operations performed out of the contact of mercury, water, or any substances that could interfere with the results.

I at first employed potassium procured by electricity; but

* A representation of the instruments and apparatus is annexed.

I soon substituted for it the metal obtained by the action of ignited iron upon potash, in the happy method discovered by M. M. GAY LUSSAC and THENARD, finding that it gave the same results, and could be obtained of an uniform quality,* and in infinitely larger quantities, and with much less labour and expense.

When ammonia is brought in contact with about twice its weight of potassium at common temperatures, the metal loses its lustre and becomes white, there is a slight diminution in the volume of the gas; but no other effects are produced. The white crust examined proves to be potash, and the ammonia is found to contain a small quantity of hydrogene, usually not more than equal in volume to the metal. On heating the potassium in the gas, by means of a spirit lamp applied to the bottom of the retort, the colour of the crust is seen to change from white to a bright azure, and this gradually passes through shades of bright blue and green into dark olive. The crust and the metal then fuse together; there is a considerable effervescence, and the crust passing off to the sides, suffers the brilliant surface of the potassium to appear. When the potassium is cooled in this state it is again covered with the white crust. By heating a second time, it swells considerably, becomes porous, and appears crystallized, and of a beautiful

* When the potash used for procuring potassium in this operation was very pure, and the iron turnings likewise very pure and clean, and the whole apparatus free from any foreign matters, the metal produced differed very little, in its properties, from that obtained by the VOLTAIC battery. Its lustre, ductility, and inflammability were similar. Its point of fusion and specific gravity were, however, a little higher it requiring nearly 130° of FAHRENHEIT to render it perfectly fluid, and being to water as 7960 to 10000, at 60° FAHRENHEIT. This I am inclined to attribute to its containing a minute proportion of iron.

azure tint; the same series of phenomena, as those before described, occur in a continuation of the process, and it is finally entirely converted into the dark olive coloured substance.

In this operation, as has been stated by M. M. GAY LUSSAC and THENARD, a gas which gives the same diminution by detonation with oxygene, as hydrogene is evolved, and ammonia disappears.

The proportion of the ammonia which looses its elastic form, as I have found by numerous trials, varies according as the gas employed contains more or less moisture.

Thus eight grains of potassium, during its conversion into the olive coloured substance, in ammonia saturated with water at 69° FAHRENHEIT, and under a pressure equal to that of 29.8 inches of mercury, had caused the disappearance of twelve cubical inches and a half of ammonia; but the same quantity of metal acted upon under similar circumstances, except that the ammonia had been deprived of as much moisture as possible by exposure for two days to potash that had been ignited, occasioned a disappearance of sixteen cubical inches of the volatile alkali.

Whatever be the degree of moisture of the gas, the quantities of inflammable gas generated have always appeared to me to be equal for equal quantities of metal. M. M. GAY LUSSAC and THENARD are said to have stated, that the proportions in their experiment were the same as would have resulted from the action of water upon potassium. In my trials, they have been rather less. Thus, in an experiment conducted with every possible attention to accuracy of manipulation, eight grains of potassium generated, by their operation upon water,

eight cubical inches and a half of hydrogene gas: and eight grains from the same mass, by their action upon ammonia, produced eight cubical inches and one eighth of inflammable gas. This difference is inconsiderable, yet I have always found it to exist, even in cases where the ammonia has been in great excess, and every part of the metal apparently converted into the olive coloured substance.

No other account of the experiments of M. M. GAY LUSSAC and THENARD has, I believe, as yet been received in this country, except that in the Moniteur already referred to; and in this no mention is made of the properties of the substance produced by the action of ammonia on potassium. Having examined them minutely and found them curious, I shall generally describe them.

1. It is crystallized and presents irregular facets, which are extremely dark, and in colour and lustre not unlike the protoxide of iron; it is opaque when examined in large masses, but is semi-transparent in thin films, and appears of a bright brown colour by transmitted light.

2. It is fusible at a heat a little above that of boiling water, and if heated much higher, emits globules of gas.

3. It appears to be considerably heavier than water, for it sinks rapidly in oil of sassafras.

4. It is a non-conductor of electricity.

5. When it is melted in oxygene gas, it burns with great vividness, emitting bright sparks. Oxygene is absorbed, nitrogene is emitted, and potash, which from its great fusibility seems to contain water, is formed.

6. When brought in contact with water, it acts upon it with much energy, produces heat, and often inflammation, and

evolves ammonia. When thrown upon water, it disappears with a hissing noise, and globules from it often move in a state of ignition upon the surface of the water. It rapidly effervesces and deliquesces in air, but can be preserved under naphtha, in which, however, it softens slowly, and seems partially to dissolve. When it is plunged under water filling an inverted jar, by means of a proper tube, it disappears instantly with effervescence, and the non-absorbable elastic fluid liberated is found to be hydrogene gas.

By far the greatest part of the ponderable matter of the ammonia, that disappears in the experiment of its action upon potassium, evidently exists in the dark fusible product. On weighing a tray containing six grains of potassium, before and after the process, the volatile alkali employed having been very dry, I found that it had increased more than two grains; the rapidity with which the product acts upon moisture, prevented me from determining the point with great minuteness; but I doubt not, that the weight of the olive coloured substance and of the hydrogene disengaged precisely equals the weight of the potassium, and ammonia consumed.

M. M. Gay Lussac and Thenard* are said to have procured from the fusible substance, by the application of a strong heat, two fifths of the quantity of ammonia that had disappeared in their first process, and a quantity of hydrogene and nitrogene in the proportions in which they exist in ammonia, equal to one fifth more.

* No notice is taken of the apparatus used by M. M. Gay Lussac and Thenard in the Moniteur; but, from the tenour of the details, it seems that they must have operated in glass vessels in the way heretofore adopted over mercury.

My results have been very different, and the reasons will, I trust, be immediately obvious.

When the retort containing the fusible substance is exhausted, filled with hydrogene and exhausted a second time, and heat gradually applied, the substance soon fuses, effervesces, and, as the heat increases, gives off a considerable quantity of elastic fluid, and becomes at length, when the temperature approaches nearly to dull redness, a dark gray solid, which, by a continuance of this degree of heat, does not undergo any alteration.

In an experiment, in which eight grains of potassium had absorbed sixteen cubical inches of well dried ammonia in a glass retort, the fusible substance gave off twelve cubical inches and half of gas, by being heated nearly to redness, and this gas analysed, was found to consist of three quarters of a cubical inch of ammonia, and the remainder of elastic fluids, which when mixed with oxygene gas in the proportion of $6\frac{1}{2}$ to 6, and acted upon by the electric spark diminished to $5\frac{1}{2}$. The temperature of the atmosphere, in this process, was 57° FAHRENHEIT, and the pressure equalled that of 30.1 inches of mercury.

In a similar experiment, in which the platina tray containing the fusible substance was heated in a polished iron tube, filled with hydrogene gas, and connected with a pneumatic apparatus containing very dry mercury, the quantity of elastic fluid given off all the corrections being made, equalled thirteen cubical inches and three quarters, and of these a cubical inch was ammonia; and the residual gas, and the gas introduced into the tube being accounted for, it appeared that the elastic fluid generated, destructible by detonation with

oxygene, was to the indestructible elastic fluid, as 2.5 to 1.

In this process, the heat applied approached to the dull red heat. The mercury, in the thermometer, stood at 62° FAHRENHEIT, and that in the barometer at 30.3 inches.

In various experiments on different quantities of the fusible substance, in some of which the heat was applied to the tray in the green glass retort, and in others, after it had been introduced into the iron tube; and in which the temperature was sometimes raised slowly and sometimes quickly, the comparative results were so near these that I have detailed, as to render any statement of them superfluous.

A little more ammonia, and rather a larger proportion of inflammable gas,* were in all instances evolved when the iron tube was used, which I am inclined to attribute to the following circumstances. When the tray was brought through the atmosphere to be introduced into the iron tube, the fusible substance absorbed a small quantity of moisture from the air, which is connected with the production of ammonia. And in the process of heating in the retort, the green glass was blackened, and I found that it contained a very small quantity of the oxides of lead and iron, which must have caused the disappearance of a small quantity of hydrogene.

M. M. GAY LUSSAC and THENARD, it appears from the statement, had brought the fusible substance in contact with mercury, which must have given to it some moisture; and whenever this is the case, it furnishes by heat variable quantities

* The average of six experiments made in a tube of iron, is 2.4 of inflammable gas to 1 of uninflammable. The average of three made in green glass retorts, is 2.3 to 1.

of ammonia. In one instance, in which I heated the fusible substance from nine grains of potassium, in a retort that had been filled with mercury in its common state of dryness, I obtained seven cubical inches of ammonia as the first product; and in another experiment which had been made with eight grains, and in which moisture was purposely introduced, I obtained nearly nine cubical inches of ammonia, and only four of the mixed gases.

I am inclined to believe, that if moisture could be introduced only in the proper proportion, the quantity of ammonia generated, would be exactly equal to that which disappeared in the first process.

This idea is confirmed by the trials which I have made, by heating the fusible substance with potash, containing its water of crystallization, and muriate of lime partially dried.*

In both these cases, ammonia was generated with great rapidity, and no other gas, but a minute quantity of inflammable gas, evolved, which was condensed by detonation with oxygene with the same phenomena as pure hydrogene.

In one instance, in which thirteen cubical inches of ammonia had disappeared, I obtained nearly eleven and three quarters by the agency of the water of the potash; the quantity of inflammable gas generated, was less than four tenths of a cubical inch.

In another, in which fourteen cubical inches had been

* If water, in its common form, is brought in contact with the fusible substance, it is impossible to regulate the quantity, so as to gain conclusive results, and a very slight *excess* of water causes the disappearance of a very large quantity of the ammonia generated. In potash and muriate of lime, in certain states of dryness, the water is too strongly attracted by the saline matter to be given off, except for the purpose of *generating* the ammonia.

absorbed, I procured by the operation of the moisture of muriate of lime, nearly eleven cubical inches of volatile alkali, and half a cubical inch of inflammable gas; and the differences, there is every reason to believe, were owing to an excess of water in the salts, by which some of the gas was absorbed.

Whenever, in experiments on the fusible substance, it has been procured from ammonia saturated with moisture, I have always found that more ammonia is generated from it by mere heat; and the general tenour of the experiments incline me to believe, that the small quantity, produced in experiments performed in vacuo, is owing to the small quantity of moisture furnished by the hydrogene gas introduced, and that the fusible substance, heated out of the presence of moisture, is incapable of producing volatile alkali.

M. M. Gay Lussac and Thenard, it is stated, after having obtained three fifths of the ammonia or its elements that had disappeared in their experiment, by heating the product; procured the remaining two fifths, by adding water to the residuum, which after this operation was found to be potash. No notice is taken of the properties of this residuum, which as the details seem to relate to a single experiment, probably was not examined; nor as moisture was present at the beginning of their operations could any accurate knowledge of its nature have been gained.

I have made the residuum of the fusible substance after it has been exposed to a dull red heat, out of the contact of moisture, an object of particular study, and I shall detail its general properties.

It was examined under naphtha, as it is instantly destroyed by the contact of air.

1. Its colour is black, and its lustre not much inferior to that of plumbago.

2. It is opaque even in the thinnest films.

3. It is very brittle, and affords a deep gray powder.

4. It is a conductor of electricity.

5. It does not fuse at a low red heat, and when raised to this temperature, in contact with plate glass, it blackens the glass, and a grayish sublimate rises from it, which likewise blackens the glass.

6. When exposed to air at common temperatures, it usually takes fire immediately, and burns with a deep red light.

7. When it is acted upon by water, it heats, effervesces most violently, and evolves volatile alkali, leaving behind nothing but potash. When the process is conducted under water, a little inflammable gas is found to be generated. A residuum of eight grains giving in all cases about $\frac{20}{100}$ of a cubical inch.

8. It has no action upon quicksilver.

9. It combines with sulphur and phosphorus by heat, without any vividness of effect, and the compounds are highly inflammable, and emit ammonia, and the one phosphuretted and the other sulphuretted hydrogene gas, by the action of water.

As an inflammable gas alone, having the obvious properties of hydrogene is given off during the action of potassium upon ammonia, and as nothing but gases apparently the same as hydrogene and nitrogene, nearly in the proportions in which they exist in volatile alkali, are evolved during the exposure of the compound to the degree of heat which I have specified; and as the residual substance produces ammonia with a little hydrogene by the action of water, it occurred to

me, that, on the principles of the antiphlogistic theory, it ought to be a compound of potassium, a little oxygene and nitrogene, or a combination of a suboxide of potassium and nitrogene; for the hydrogene disengaged in the operations of which it was the result, nearly equalled the whole quantity contained in the ammonia employed; and it was easy to explain the fact of the reproduction of the ammonia by water, on the supposition, that by combination with one portion of the oxygene of the water, the oxide of potassium became potash, and by combination with another portion and its hydrogene, the nitrogene was converted into volatile alkali.

With a view to ascertain this point, I made several experiments on various residuums, procured in the way that I have just stated, from the action of equal quantities of potassium on dry ammonia in platina trays, each portion of metal equalling six grains.

In the first trials, I endeavoured to ascertain the quantity of ammonia generated by the action of water upon a residuum, by heating it with muriate of lime or potash partially deprived of moisture; and after several trials, many of which failed, I succeeded in obtaining four cubical inches and a half of ammonia. In three other cases, where there was reason to suspect a small excess of water, the quantities of ammonia were three cubical inches and a half, three and eight tenths, and four and two tenths.

These experiments were performed in the iron tube used for the former process; the tray was not withdrawn; but the salt introduced in powder, and the apparatus exhausted as before, then filled with hydrogene, and then gently heated in a small portable forge.

Having ascertained what quantity of ammonia was given off from the residuum, I endeavoured to discover what quantity of nitrogene it produced in combustion, and what quantity of oxygene it absorbed. The methods that I employed, were by introducing the trays into vessels filled with oxygene gas over mercury. The product often inflamed spontaneously, and could always be made to burn by a slight degree of heat.

In the trial that I regard as the most accurate, two cubical inches and a half of oxygene were absorbed, and only a cubical inch and one tenth of nitrogene evolved.

Surprised at the smallness of the quantity of the nitrogene, I sought for ammonia in the products of these operations; but various trials convinced me that none was formed. I examined the solid substances produced, expecting nitrous acid; but the matter proved to be dry potash, apparently pure, and not affording the slightest traces of acid.

The quantity of nitrogene existing in the ammonia, which this residuum would have produced by the action of *water*, supposing the volatile alkali decomposed by electricity, would have equalled at least two cubical inches and a quarter.

I heated the same proportions of residuum with the red oxide of mercury, and the red oxide of lead in vacuo, expecting that when oxygene was supplied in a gradual way, the result might be different from that of combustion; but in neither of these cases did the quantity of nitrogene exceed a cubical inch and a half.

But on what could this loss of nitrogene depend; had it entered into any unknown form with oxygene, or did it not really exist in the residuum in the same quantity, as in the *ammonia* produced from it?

I hoped that an experiment of exposing the residuum to intense heat might enlighten the inquiry. I distilled one of the portions which had been covered with naphtha, in a tube of wrought platina made for the purpose. The tube had been exhausted and filled with hydrogene, and exhausted again, and was then connected with a pneumatic mercurial apparatus. Heat was at first slowly applied till the naphtha had been driven over. It was then raised rapidly by an excellent forge. When the tube became cherry red, gas was develloped; it continued to be generated for some minutes. When the tube had received the most intense heat that could be applied, the operation was stopped. The quantity of gas collected, making the proper corrections and reductions, would have been three cubical inches and a half at the mean temperature and pressure. Twelve measures of it were mixed with six of oxygene gas, the electrical spark was passed through the mixture; a strong inflammation took place, the diminution was to three measures and a half, and the residuum contained oxygene. This experiment was repeated upon different quantities with the same comparative results.

In examining the platina tube, which had a screw adapted to it at the lower extremity, by means of which it could be opened. The lower part was found to contain potash, which had all the properties of the pure alkali, and in the upper part there was a quantity of potassium. Water poured into the tube, produced a violent heat and inflammation; but no smell of ammonia.

This result was so unexpected and so extraordinary, that I at first supposed there was some source of error. I had calculated upon procuring nitrogene as the only aeriform

product; I obtained an elastic fluid which gave much more diminution by detonation with oxygene, than that produced from ammonia by electricity.

I now made the experiment, by heating the entire fusible substance, from six grains of potassium which had absorbed twelve cubical inches of ammonia, in the iron tube, in the manner before described. The heat was gradually raised to whiteness, and the gas collected in two portions. The whole quantity generated, making the usual corrections for temperature and pressure, and the portion of hydrogene originally in the tube, and the residuum, would have been fourteen cubical inches and a half at the mean degree of the barometer and thermometer. Of these, nearly a cubical inch was ammonia and the remainder a gas, of which the portion destructible by detonation with oxygene, was to the indestructible portion, as 2.7 to 1.

The lower part of the tube, where the heat had been intense, was found surrounded with potash in a vitreous form; the upper part contained a considerable quantity of potassium.

In another similar experiment, made expressly for the purposes of ascertaining the quantity of potassium recovered, the same elastic products were evolved. The tube was suffered to cool, the stop-cock being open in contact with mercury, it was filled with mercury, and the mercury displaced by water; when two cubical inches and three quarters of hydrogene gas were generated, which proved that at least two grains and a half of potassium had been revived.

Now, if a calculation be made upon the products in these operations, considering them as nitrogene and hydrogene, and taking the common standard of temperature and pressure, it

will be found, that by the decomposition of 11 cubical inches of ammonia equal to 2.05 grains, there is generated 3.6 cubical inches of nitrogene equal to 1.06 grains, and 9.9 cubical inches of hydrogene, which added to that disengaged in the first operation equal to about 6.1 cubical inches, are together equal to .982 grains; and the oxygene added to 3.5 grains of potassium would be .6 grains, and the whole amount is 2.04 grains; and 2.05 — 2.04 = .01. But the same quantity of ammonia, decomposed by electricity, would have given 5.5 cubical inches of nitrogene equal to 1.6 grains, and only 14 cubical inches of hydrogene* equal to .38, and allowing the separation of oxygene in this process in water, it cannot be estimated at more than .11 or .12.

So that if the analysis of ammonia by electricity at all approaches towards accuracy; in the process just described, there is a considerable loss of nitrogene, and a production of oxygene and inflammable gas.

And in the action of water upon the residuum, in the experiment page 52, there is an apparent generation of nitrogene.

How can these extraordinary results be explained?

The decomposition and composition of nitrogene seem proved, allowing the correctness of the data; and one of its elements appears to be oxygene; but what is its other elementary matter?

Is the gas that appears to possess the properties of hydrogene, a new species of inflammable aeriform substance?

Or has nitrogene a metallic basis which alloys with the iron or platina?

* See Phil. Trans. 1808, p. 40.

Or is water alike the *ponderable* matter of nitrogene, hydrogene, and oxygene?

Or is nitrogene a compound of hydrogene with a larger proportion of oxygene than exists in water?

These important questions, the two first of which seem the least likely to be answered in the affirmative, from the correspondence between the weight of the ammonia decomposed, and the products, supposing them to be known substances, I I shall use every effort to solve by new labours, and I hope soon to be able to communicate the results of further experiments on the subject to the Society.

As the inquiry now stands, it is however sufficiently demonstrative, that the opinion which I had ventured to form respecting the decomposition of ammonia in this experiment, is correct; and that M. M. GAY LUSSAC's and THENARD's idea of the decomposition of the potassium, and their theory of its being compounded of hydrogene and potash, are unfounded.

For a considerable part of the potassium is recovered unaltered, and in the entire decomposition of the fusible substance, there is only a small excess of hydrogene above that existing in the ammonia acted upon.

The mere phenomena of the process likewise, if minutely examined, prove the same thing.

After the first slight effervescence, owing to the water absorbed by the potash formed upon the potassium during its exposure to the air, the operation proceeds with the greatest tranquillity. No elastic fluid is given off from the potassium; it often appears covered with the olive coloured substance, and if it were evolving hydrogene; this must pass through

the fluid; but even to the end of the operation, no such appearance occurs.

The crystallized and spongy substance, formed in the first part of the process, I am inclined to consider as a combination of ammonium and potassium, for it emits a smell of ammonia when exposed to air, and is considerably lighter than potassium.

I at first thought that a solid compound of hydrogene and potassium might be generated in the first part of this operation: but experiments on the immediate action of potassium and hydrogene did not favour this opinion. Potassium, as I ventured to conclude in the Bakerian Lecture for 1807,* is very

* M. M. Gay Lussac and Thenard seem to be of a different opinion. In the Moniteur, to which I have so often referred, it is related, that these distinguished chemists, exposing hydrogene to potassium at a high temperature, found that the hydrogene was absorbed, and that it formed a compound with the potassium of a light gray colour, from which hydrogene was capable of being obtained by the action of water or mercury.

After a number of trials, I have not been able to witness this result. In an experiment which I made in the presence of Mr. Pepys, and which I have often repeated, and twice before a numerous assembly, in retorts of plate glass, four grains of potassium were heated in fourteen cubical inches of pure hydrogene. At first, white fumes arose and precipitated themselves in the neck of the retort. When a considerable film of the precipitate had collected, its colour appeared a bright gray, and after the first two or three minutes, it ceased to be formed.

The bottom of the retort was heated to redness, when the potassium began to sublime and condense on the sides.

The process was stopped, and the retort suffered to cool. The absorption was not equal to a quarter of a cubical inch. When the retort was *broken*, the gas in passing into the atmosphere, produced an explosion with most vivid light, and white fumes. The potassium remaining in the retort, and that which had sublimed, seemed unaltered in their properties.

The grayish substance inflamed by the action of water, but did not seem to be combinable with mercury. I am inclined to attribute its formation to the agency of

soluble in hydrogene; but, under common circumstances, hydrogene does not seem to be absorbable by potassium.

moisture suspended in the hydrogene, and to consider it as a triple compound of potassium, oxygene, and hydrogene.

When potassium is heated in a gas containing hydrogene, and from $\frac{1}{7}$ to $\frac{1}{10}$ of common air, it is formed in greater quantities, and a crust of it covers the metal, and in the process there is an absorption both of hydrogene and oxygene. It is likewise produced in experiments on the generation of potassium by exposing potash to ignited iron, at the time (I believe) that common air is admitted, during the cooling of the tube.

It is non-conducting, inflames spontaneously in air, and produces potash and aqueous vapour by its combustion.

When potassium is heated in hydrogene in a flint glass retort, or even for a great length of time in a green glass retort, there is an absorption of the gas; but this is independent of the presence of potassium, and is owing to the action of the metallic oxides in the glass upon the hydrogene.

If a solid compound of hydrogene and potassium could be formed, we might expect its existence in the experiment with the gun barrel, in which potassium is exposed to hydrogene at almost every temperature; but the metal formed in this process, when proper precautions are taken to exclude carbonaceous matters, is uniform in its properties, and generates for equal quantities, equal proportions of hydrogene by the action of water.

The general phenomena of this operation, shew indeed that the solution of potassium in hydrogene is intimately connected with the general principle of the decomposition, and confirm my first idea of the action of the two bodies.

Hydrogene dissolves a large quantity of potassium by heat, but the greater portion is precipitated on cooling. The attractions which determine the chemical change, seem to be that of iron for oxygene, of iron for potassium, and of hydrogene for potassium; and in experiments, in which a very intense heat is used for the production of potassium by iron, I have often found, that the gas which comes over, though it has passed through a tube cooled by ice, inflames spontaneously in the atmosphere, and burns with a most brilliant light which is purple at the edges, and throws off a dense vapour containing potash.

Sodium appears to be almost insoluble in hydrogene, and this seems to be one reason why it cannot be obtained, except in very minute quantities, in the experiment with the gun barrel.

Sodium, though scarcely capable of being dissolved in hydrogene alone, seems to be

3. *Analytical Experiments on Sulphur.*

I have referred, on a former occasion,* to the experiments of Mr. Clayfield and of Mr. Berthollet, jun. which seemed to show that sulphur, in its common form, contained hydrogene. In considering the analytical powers of the Voltaic apparatus, it occurred to me, that though sulphur, from its being a non-conductor, could not be expected to yield its elements to the electrical attractions and repulsions of the opposite surfaces, yet that the intense heat, connected with the contact of these surfaces, might possibly effect some alteration in it, and tend to separate any elastic matter it might contain.

On this idea some experiments were instituted in 1807. A curved glass tube, having a platina wire hermetically sealed in its upper extremity, was filled with sulphur. The sulphur was melted over a spirit lamp; and a proper connection being made with the Voltaic apparatus of one hundred plates of six inches, in great activity, a contact was made in the sulphur by means of another platina wire. A most brilliant spark, which appeared orange coloured through the sulphur, was produced, and a minute portion of elastic fluid rose to the upper extremity of the tube. By a continuation of the

soluble in the compound of hydrogene and potassium. By exposing mixtures of potash and soda, to ignited iron I have obtained some very curious alloys; which, whether the potassium or the sodium was in excess, were fluid at common temperatures. The compound containing an excess of potassium was even lighter than potassium (probably from its fluidity). All these alloys were in the highest degree inflammable. When a globule of the fluid alloy was touched by a globule of mercury, they combined with a heat that singed the paper upon which the experiment was made, and formed, when cool, a solid so hard, as not to be cut by a knife.

* Bakerian Lecture, 1808, p. 16.

process for nearly an hour, a globule equal to about the tenth of an inch in diameter was obtained, which, when examined, was found to be sulphuretted hydrogene.

This result perfectly coincided with those which have been just mentioned; but as the sulphur that I had used was merely in its common state, and as the ingenious experiments of Dr. THOMSON have shewn that sulphur in certain forms may contain water, I did not venture, at that time, to form any conclusion upon the subject.

In the summer of the present year, I repeated the experiment with every precaution. The sulphur that I employed was Sicilian sulphur, that had been recently sublimed in a retort filled with nitrogene gas, and that had been kept hot till the moment that it was used. The power applied was that of the battery of five hundred double plates of six inches, highly charged. In this case the action was most intense, the heat strong, and the light extremely brilliant; the sulphur soon entered into ebullition, elastic matter was formed in great quantities, much of which was permanent; and the sulphur, from being of a pure yellow, became of a deep red brown tint.

The gas, as in the former instance, proved to be sulphuretted hydrogene. The platina wires were considerably acted upon; the sulphur, at its point of contact with them, had obtained the power of reddening moistened litmus paper.

I endeavoured to ascertain the quantity of sulphuretted hydrogene evolved in this way from a given quantity of sulphur, and for this purpose, I electrized a quantity equal to about two hundred grains in an apparatus of the kind I have just

described, and when the upper part of the tube was full of gas, I suffered it to pass into the atmosphere; so as to enable me to repeat the process.

When I operated in this way, there seemed to be no limit to the generation of elastic fluid, and in about two hours a quantity had been evolved, which amounted to more than five times the volume of the sulphur employed. From the circumstances of the experiment, the last portion only could be examined, and this proved to be sulphuretted hydrogene. Towards the end of the process, the sulphur became extremely difficult of fusion, and almost opaque, and when cooled and broken, was found of a dirty brown colour.

The experiments upon the union of sulphur and potassium, which I laid before the Society last year, prove that these bodies act upon each other with great energy, and that sulphuretted hydrogene is evolved in the process, with intense heat and light.

In heating potassium in contact with compound inflammable substances, such as resin, wax, camphor, and fixed oils in close vessels out of the contact of the air, I found that a violent inflammation was occasioned, that hydrocarbonate was evolved; and that when the compound was not in great excess, a substance was formed, spontaneously inflammable at common temperatures, the combustible materials of which were charcoal and potassium.

Here was a strong analogy between the action of these bodies, and sulphur on potassium. Their physical properties likewise resemble those of sulphur; for they agree in being non-conductors, whether fluid or solid, in being transparent when fluid, and semi-transparent when solid, and highly

refractive; their affections by electricity are likewise similar to those of sulphur; for the oily bodies give out hydrocarbonate by the agency of the VOLTAIC spark, and become brown, as if from the deposition of carbonaceous matter.

But the resinous and oily substances are compounds of a small quantity of hydrogene and oxygene, with a large quantity of a carbonaceous basis. The existence of hydrogene in sulphur is fully proved, and we have no right to consider a substance, which can be produced from it in such large quantities, merely as an accidental ingredient.

The oily substances in combustion, produce two or three times their weight of carbonic acid and some water; I endeavoured to ascertain whether water was formed in the combustion of sulphur in oxygene gas, dried by exposure to potash: but in this case sulphureous acid is produced in much larger quantities than sulphuric acid, and this last product is condensed with great difficulty. In cases, however, in which I have obtained, by applying artificial cold, a deposition of acid in the form of a film of dew in glass retorts out of the contact of the atmosphere, in which sulphur had been burned in oxygene gas hygrometrically dry, it has appeared to me less tenacious and lighter than the common sulphuric acid of commerce, which in the most concentrated form in which I have seen it, namely, at 1.855, gave abundance of hydrogene as well as sulphur, at the negative surface in the VOLTAIC circuit, and hence evidently contained water.

The reddening of the litmus paper, by sulphur that had been acted on by VOLTAIC electricity, might be ascribed to its containing some of the sulphuretted hydrogene formed in the process; but even the production of this gas, as will be im-

mediately seen, is an evidence of the existence of oxygene in sulphur.

In my early experiments on potassium, procured by electricity, I heated small globules of potassium in large quantities of sulphuretted hydrogene, and I found that sulphuret of potash was formed; but this might be owing to the water dissolved in the gas, and I ventured to draw no conclusion till I had tried the experiment in an unobjectionable manner.

I heated four grains of potassium in a retort of the capacity of twenty cubical inches; it had been filled after the usual processes of exhaustion with sulphuretted hydrogene, dried by means of muriate of lime that had been heated to whiteness; as soon as the potassium fused, white fumes were copiously emitted, and the potassium soon took fire, and burnt with a most brilliant flame, yellow in the centre and red towards the circumference.*

The diminution of the volume of the elastic matter, in this operation, did not equal more than two cubical inches and a half. A very small quantity of the residual gas only was absorbable by water. The non-absorbable gas was hydrogene, holding a minute quantity of sulphur in solution.

A yellow sublimate lined the upper part of the retort, which proved to be sulphur. The solid matter formed was red at the surface like sulphuret of potash, but in the interior it was dark gray, like sulphuret of potassium. The piece of the retort containing it was introduced into a jar inverted over mercury,

* In the Moniteur, May 27, 1808, in the account of M. M. Gay Lussac's and Thenard's experiments, it is mentioned, that potassium absorbs the sulphur and a part of the hydrogene of sulphuretted hydrogene; but the phenomena of inflammation is not mentioned, nor are the results described.

and acted upon by a small quantity of dense muriatic acid, diluted with an equal weight of water, when there were disengaged two cubical inches and a quarter of gas, which proved to be sulphuretted hydrogene.

In another experiment, in which eight grains of potassium were heated in a retort of the capacity of twenty cubical inches, containing about nineteen cubical inches of sulphuretted hydrogene, and a cubical inch of phosphuretted hydrogene, which was introduced for the purpose of absorbing the oxygene of the small quantity of common air admitted by the stop-cock, the inflammation took place as before, there was a similar precipitation of sulphur on the sides of the retort; the mass formed in the place of the potassium was orange externally, and of a dark gray colour internally, as in the last instance; and when acted on by a little water holding muriatic acid in solution, there were evolved from it five cubical inches only of sulphuretted hydrogene.

Both these experiments concur in proving the existence of a principle in sulphuretted hydrogene, capable of destroying partially the inflammability of potassium, and of producing upon it all the effects of oxygene; for had the potassium combined merely with pure combustible matter, it ought, as will be seen distinctly from what follows, to have evolved by the action of the acid, a volume of sulphuretted hydrogene, at least equal to that of the hydrogene, which an equal weight of uncombined potassium would have produced by its operation upon water.

Sulphuretted hydrogene, as has been long known to chemists, may be formed by heating sulphur strongly in hydrogene gas. I heated four grains of sulphur in a glass retort,

containing about twenty cubical inches of hydrogene, by means of a spirit lamp, and pushed the heat nearly to redness. There was no perceptible change of volume in the gas after the process; the sulphur that had sublimed was unaltered in its properties, and about three cubical inches of an elastic fluid absorbable by water were formed: the solution reddened litmus, and had all the properties of a solution of pure sulphuretted hydrogene. Now if we suppose sulphuretted hydrogene to be constituted by sulphur dissolved in its unaltered state in hydrogene, and allow the existence of oxygene in this gas; its existence must likewise be allowed in sulphur, for we have no right to assume that sulphur in sulphuretted hydrogene is combined with more oxygene than in its common form: it is well known, that when electrical sparks are passed through sulphuretted hydrogene, a considerable portion of sulphur is separated without any alteration in the volume of the gas. This experiment I have made more than once, and I found that the sulphur obtained, in fusibility, combustibility, and other sensible properties did not perceptibly differ from common sublimed sulphur.

According to these ideas, the intense ignition produced by the action of sulphur, on potassium and sodium, must not be ascribed merely to the affinity of the metals of the alkalies for its basis, but may be attributed likewise to the agency of the oxygene that it contains.

The minute examination of the circumstances of the action of potassium and sulphur likewise confirms these opinions.

When two grains of potassium and one of sulphur were heated gently in a green glass tube filled with hydrogene, and connected with a pneumatic apparatus, there was a most

intense ignition produced by the action of the two bodies, and one eighth of a cubical inch of gas was disengaged, which was sulphuretted hydrogene. The compound was exposed in a mercurial apparatus to the action of liquid muriatic acid; when a cubical inch and quarter of aeriform matter was produced, which proved to be pure sulphuretted hydrogene.

The same experiment was repeated, except that four grains of sulphur were employed instead of one. In this case, a quarter of a cubical inch of gas was disengaged during the process of combination; and when the compound was acted upon by muriatic acid, only three quarters of a cubical inch of sulphuretted hydrogene was obtained.

Now, *sulphuret* of potash produces sulphuretted hydrogene by the action of an acid; and if the sulphur had not contained oxygene, the hydrogene evolved by the action of the potassium in both these experiments ought to have equalled at least two cubical inches, and the whole quantity of sulphuretted hydrogene ought to have been more: and that so much less sulphuretted hydrogene was evolved in the second experiment, can only be ascribed to the larger quantity of oxygene furnished to the potassium by the larger quantity of the sulphur.

I have made several experiments of this kind with similar results. Whenever equal quantities of potassium were combined with unequal quantities of sulphur, and exposed afterwards to the action of muriatic acid, the largest quantity of sulphuretted hydrogene was furnished by the product containing the smallest proportion of sulphur, and in no case was the quantity of gas equal in volume to the quantity of hydrogene, which would have been produced by the mere action of potassium upon water.

From the general tenour of these various facts, it will not be, I trust, unreasonable to assume, that sulphur, in its common state, is a compound of small quantities of oxygene and hydrogene with a large quantity of a basis that produces the acids of sulphur in combustion, and which, on account of its strong attractions for other bodies, it will probably be very difficult to obtain in its pure form.

In metallic combinations even, it still probably retains its oxygene and part of its hydrogene. Metallic sulphurets can only be partially decomposed by heat, and the small quantity of sulphur evolved from them in this case when perfectly dry and out of the contact of air, as I found in an experiment on the sulphurets of copper and iron, exists in its common state, and acts upon potassium, and is affected by electricity in the same manner as native sulphur.

4. Analytical Experiments on Phosphorus.

The same analogies apply to phosphorus as to sulphur, and I have made a similar series of experiments on this inflammable substance.

Common electrical sparks, passed through phosphorus, did not evolve from it any permanent gas; but when it was acted upon by the VOLTAIC electricity of the battery of five hundred plates in the same manner as sulphur, gas was produced in considerable quantities, and the phosphorus became of a deep red brown colour, like phosphorus that has been inflamed and extinguished under water. The gas examined proved to be phosphuretted hydrogene, and in one experiment, continued for some hours, a quantity estimated to be nearly equal to four times the volume of the phosphorus employed was given

off. The light of the VOLTAIC spark in the phosphorus was at first a brilliant yellow, but as the colour of the phosphorus changed, it appeared orange.

I heated three grains of potassium in sixteen cubical inches of phosphuretted hydrogene; as soon as it was fused, the retort became filled with white fumes, and a reddish substance precipitated upon the sides and upper part of it. The heat was applied for some minutes. No inflammation took place.* When the retort was cool, the absorption was found to be less than a cubical inch. The potassium externally was of a deep brown colour, internally it was of a dull lead colour. The residual gas had lost its property of spontaneous inflammation, but seemed still to contain a small quantity of phosphorus in solution.

The phosphuret acted upon over-mercury by solution of muriatic acid evolved only one cubical inch and three quarters of phosphuretted hydrogene.

From this experiment, there is great reason to suppose that phosphuretted hydrogene contains a minute proportion of oxygene, and consequently that phosphorus likewise may contain it; but the action of potassium on phosphorus itself furnishes perhaps more direct evidences of the circumstance.

One grain of potassium and one grain of phosphorus were fused together in a proper apparatus. They combined with the production of the most vivid light and intense ignition. During the process one tenth of a cubical inch of phosphuretted hydrogene was evolved. The phosphuret formed, exposed

* It is stated, in the account before referred to of M. M. GAY LUSSAC's and THENARD's experiments, that potassium inflames in phosphuretted hydrogene. My experiments upon this gas have been often repeated. I have never perceived any luminous appearance; but I have always operated in day-light.

to the action of diluted muriatic acid over mercury, produced exactly three tenths of a cubical inch of phosphuretted hydrogene.

In a second experiment, one grain of potassium was fused with three grains of phosphorus; in this case nearly a quarter of a cubical inch of phosphuretted hydrogene was generated during the ignition. But from the compound exposed to muriatic acid, only one tenth of a cubical inch could be procured.

Now it is not easy to refer the deficiency of phosphuretted hydrogene in the second case to any other cause than to the supply of oxygene to the potassium from the phosphorus; and the quantity of phosphuretted hydrogene evolved in the first case, is much less than could be expected, if both potassium and phosphorus consisted merely of pure combustible matter.

The phosphoric acid, formed by the combustion of phosphorus, though a crystalline solid, may still contain water. The hydrogene evolved from phosphorus by electricity proves indeed that this must be the case; and though the quantity of hydrogene and oxygene in phosphorus may be exceedingly small, yet they may be sufficient to give it peculiar characters; and till the basis is obtained free, we shall have no knowledge of the properties of the pure phosphoric element.

5. *On the States of the carbonaceous Principle in Plumbago, Charcoal, and the Diamond.*

The accurate researches of Messrs. ALLEN and PEPYS have distinctly proved, that plumbago, charcoal, and the diamond

produce very nearly the same quantities of carbonic acid, and absorb very nearly the same quantities of oxygene in combustion.

Hence it is evident, that they must consist principally of the same kind of elementary matter; but minute researches upon their chemical relations, when examined by new analytical methods will, I am inclined to believe, shew that the great difference in their physical properties does not merely depend upon the differences of the mechanical arrangement of their parts, but likewise upon differences in their intimate chemical nature.

I endeavoured to discover, whether any elastic matter could be obtained from plumbago very intensely ignited by the VOLTAIC battery in a Torricellian vacuum: but though the highest power of the battery of five hundred was employed, and though the heat was such, as in another experiment instantly melted platina wire of $\frac{1}{50}$th of an inch in diameter, yet no appearance of change took place upon the plumbago. Its characters remained wholly unaltered, and no permanent elastic fluid was formed.

I heated one grain of plumbago, with twice its weight of potassium, in a plate glass tube connected with a proper apparatus, and I heated an equal quantity of potassium alone in a tube of the same kind, for an equal length of time, namely, eight minutes. Both tubes were filled with hydrogene: no gas was evolved in either case. There was no ignition in the tube containing the plumbago, but it seemed gradually to combine with the potassium. The two results were exposed to the action of water; the result from the plumbago acted upon that fluid with as much energy as the other result, and

the two volumes of elastic fluids were 1.8 cubical inches and 1.9 cubical inches; and both gave the same diminution by detonation with oxygene, as pure hydrogene. Two grains of potassium, by acting upon water, would have produced two cubical inches and one eighth of hydrogene gas; the deficiency in the result, in which potassium alone was used, must be ascribed to the loss of a small quantity of metal, which must have been carried off in solution in the hydrogene, and perhaps, likewise, to the action of the minute quantity of metallic oxides in the plate glass. The difference in the quantity of hydrogene given off in the two results, is however too slight to ascribe it to the existence of oxygene in the plumbago.

I repeated this experiment several times with like results, and in two or three instances examined the compound formed. It was infusible at a red heat, had the lustre of plumbago. It inflamed spontaneously, when exposed to air, generated potash, and left a black powdery residuum. It effervesced most violently in water, and produced a gas, which burnt like pure hydrogene.

When small pieces of charcoal from the willow, that had been intensely ignited, were acted upon by VOLTAIC electricity in a Torricellian vacuum, every precaution being taken to exclude moisture from the mercury and the charcoal, the results were very different from those occuring in the case of plumbago.

When plumbago was used, after the first spark, which generally passed through a distance of about one eighth of an inch, there was no continuation of light, without a contact or an approach to the same distance; but from the charcoal a

flame seemed to issue of a most brilliant purple, and formed, as it were, a conducting chain of light of nearly an inch in length, at the same time that elastic matter was rapidly formed, some of which was permanent. After many unsuccessful trials, I at length succeeded in collecting the quantity of elastic fluid given out by half a grain of charcoal; the process had been continued nearly half an hour. The quantity of gas amounted to nearly an eighth of a cubical inch; it was inflammable by the electric spark with oxygene gas, and four measures of it absorbed three measures of oxygene, and produced one measure and a half of carbonic acid. The charcoal in this experiment had become harder at the point, and its lustre, where it had been heated to whiteness, approached to that of plumbago.

I heated two grains of potassium together with two grains of charcoal, for five minutes; and to estimate the effects of the metallic oxides and potash in the green glass tube, I made a comparative experiment, as in the case of plumbago; but there was no proof of any oxygene being furnished to the potassium from the charcoal in the process, for the compound acted upon water with great energy, and produced a quantity of inflammable gas, only inferior by one twelfth to that produced by the potassium, which had not been combined with charcoal, and which gave the same diminution by detonation with oxygene; and the slight difference may be well ascribed to the influence of foreign matters in the charcoal. There was no ignition in the process, and no gas was evolved.

The compound produced in other experiments of this kind was examined. It is a conductor of electricity, is of a dense

black, inflames spontaneously, and burns with a deep red light in the atmosphere.*

The non-conducting nature of the diamond and its infusibility, rendered it impossible to act upon it by VOLTAIC electricity; and the only new agents which seemed to offer any means of decomposing it, were the metals of the alkalies.

When a diamond is heated in a green glass tube with potassium, there is no elastic fluid given out, and no intensity of action; but the diamond soon blackens, and scales seem to detach themselves from it, and these scales, when examined in the magnifier, are gray externally, and of the colour of plumbago internally, as if they consisted of plumbago covered by the gray oxide of potassium.

In heating together three grains of diamonds in powder, and two grains of potassium, for an hour in a small retort of plate glass filled with hydrogene, and making the comparative trial with two grains of potassium heated in a similar apparatus, without any diamonds, I found that the potassium which had been heated with the diamonds, produced, by its action upon water, one cubical inch and $\frac{1}{10}$ of inflammable air, and that which had been exposed to heat alone, all other circumstances being similar, evolved nearly one cubical inch and $\frac{1}{10}$, both of which were pure hydrogene.

In another experiment of a similar kind, in which fragments of diamonds were used in the quantity of four grains, the potassium became extremely black from its action upon them during an exposure to heat for three hours, and the diamonds

* In the Bakerian Lecture for 1807, I have mentioned the decomposition of carbonic acid by potassium, which takes place with inflammation. If the potassium is in excess in this experiment, the same pyrophorus as that described above is formed.

were covered with a grayish crust, and when acted upon by water and dried, were found to have lost about $\frac{11}{100}$ of a grain in weight. The matter separated by washing, and examined, appeared as a fine powder of a dense black colour. When a surface of platina wire was covered with it, and made to touch another wire in the VOLTAIC circuit, a brilliant spark with combustion occurred. It burnt, when heated to redness in a green glass tube filled with oxygene gas, and produced carbonic acid by its combustion.

These general results seem to shew, that in plumbago the carbonaceous element exists merely in combination with iron, and in a form which may be regarded as approaching to that of a metal in its nature, being conducting in a high degree, opaque, and possessing considerable lustre.

Charcoal appears to contain a minute quantity of hydrogene in combination. Possibly likewise, the alkalies and earths produced during its combustion, exist in it not fully combined with oxygene, and according to these ideas, it is a very compounded substance, though in the main it consists of the pure carbonaceous element.

The experiments on the diamond render it extremely likely that it contains oxygene; but the quantity must be exceedingly minute, though probably sufficient to render the compound non-conducting: and if the carbonaceous element in charcoal and the diamond be considered as united to still less foreign matter in quantity, than in plumbago, which contains about $\frac{1}{20}$ of iron, the results of their combustion, as examined independently of hygrometrical tests, will not differ perceptibly.

Whoever considers the difference between iron and steel, in which there does not exist more than $\frac{1}{200}$ of plumbago, or the

difference between the amalgam of ammonium, and mercury, in which the quantity of new matter is not more than $\frac{1}{12000}$, or that between the metals and their sub-oxides, some of which contain less than $\frac{1}{10}$ of oxygene, will not be disposed to question the principle, that minute differences in chemical composition may produce great differences in external and physical characters.

6. *Experiments on the Decomposition, and Composition of the Boracic Acid.*

In the last Bakerian Lecture,* I have given an account of an experiment in which boracic acid appeared to be decomposed by VOLTAIC electricity, a dark coloured inflammable substance separating from it on the negative surface.

In the course of the spring and summer, I made many attempts to collect quantities of this substance for minute examination. When boracic acid, moistened with water, was exposed between two surfaces of platina, acted on by the full power of the battery of five hundred, an olive-brown matter immediately began to form on the negative surface, which gradually increased in thickness, and at last appeared almost black. It was permanent in water, but soluble with effervescence in warm nitrous acid. When heated to redness upon the platina it burnt slowly, and gave off white fumes, which slightly reddened moistened litmus paper, and it left a black mass, which, when examined by the magnifier, appeared vitreous at the surface, and evidently contained a fixed acid.

These circumstances seemed distinctly to shew the decomposition, and recomposition of the boracic acid; but as the

* Page 43.

peculiar combustible substance was a non-conductor of electricity, I was never able to obtain it, except in very thin films upon the platina. It was not possible to examine its properties minutely, or to determine its precise nature, or whether it was the pure boracic basis; I consequently endeavoured to apply other methods of decomposition, and to find other more unequivocal evidences upon this important chemical subject.

I have already laid before the Society an account of an experiment,* in which boracic acid, heated in contact with potassium in a gold tube, was converted into borate of potash, at the same time that a dark coloured matter, similar to that produced from the acid by electricity, was formed. About two months after this experiment had been made, namely, in the beginning of August, at a time that I was repeating the process, and examining minutely the results, I was informed, by a letter from Mr. CADELL at Paris, that M. THENARD was employed in the decomposition of the boracic acid by potassium, and that he had heated the two substances together in a copper tube, and had obtained borate of potash, and a peculiar matter concerning the nature of which no details were given in the communication.

That the same results must be obtained by the same methods of operating, there could be no doubt. The evidences for the decomposition of the boracic acid are easily gained, the synthetical proofs of its nature involve more complicated circumstances.

I found that when equal weights of potassium and boracic acid were heated together in a green glass tube, which had been exhausted after having been twice filled with hydro-

* Phil. Trans. Part II. 1808. p. 343.

gene, there was a most intense ignition before the temperature was nearly raised to the red heat; the potassium entered into vivid inflammation, where it was in contact with the boracic acid. When this acid had been heated to whiteness, before it was introduced into the tube, and powdered and made use of whilst yet warm, the quantity of gas given out in the operation did not exceed twice the volume of the acid, and was hydrogene.

I could only use twelve or fourteen grains of each of the two substances in this mode of conducting the experiment; for when larger quantities were employed, the glass tube always ran into fusion from the intensity of the heat produced during the action.

When the film of naphtha had not been carefully removed from the potassium, the mass appeared black throughout; but when this had been the case, the colour was of a dark olive-brown.

In several experiments, in which I used equal parts of the acid and metal, I found that there was always a great quantity of the former in the residuum, and by various trials, I ascertained that twenty grains of potassium had their inflammability entirely destroyed by about eight grains of boracic acid.

For collecting considerable portions of the matters formed in the process, I used metallic tubes furnished with stop-cocks, and exhausted after being filled with hydrogene.

When tubes of brass or copper were employed, the heat was only raised to a dull red; but when iron tubes were used, it was pushed to whiteness. In all cases the acid was decomposed, and the products were scarcely different.

When the result was taken out of a tube of brass or copper,

it appeared as an olive coloured glass, having opaque, dull olive-brown specks diffused through it.

It gave a very slight effervescence with water, and partially dissolved in hot water, a dark olive coloured powder separating from it.

The results from the iron tube, which had been much more strongly heated, were dark olive in some parts, and almost black in others. They did not effervesce with warm water, but were rapidly acted upon by it, and the particles separated by washing, were of a shade of olive, so dark as to appear almost black on white paper.

The solutions obtained, when passed through a filter, had a faint olive tint, and contained sub-borate of potash, and potash. In cases, when instead of water, a weak solution of muriatic acid was used for separating the saline matter, from the inflammable matter, the fluid came through the filter colourless.

In describing the properties of the new inflammable substance separated by washing, I shall speak of that collected from operations conducted in tubes of brass, in the manner that has been just mentioned; for it is in this way, that I have collected the largest quantities.

It appears as a pulverulent mass of the darkest shades of olive. It is perfectly opaque. It is very friable, and its powder does not scratch glass. It is a non-conductor of electricity.

When it has been dried only at 100 or 120°, it gives off moisture by increase of temperature, and if heated in the atmosphere, takes fire at a temperature below the boiling point of olive oil, and burns with a red light and scintillations like charcoal.

If it be excluded from air and heated to whiteness in a tube of platina, exhausted after having been filled with hydrogene, it is found very little altered after the process. Its colour is a little darker, and it is rather denser; but no indications are given of any part of it having undergone fusion, volatilization, or decomposition. Before the process its specific gravity is such that it does not sink in sulphuric acid; but after, it rapidly falls to the bottom in this fluid.

The phenomena of its combustion, are best witnessed in a retort filled with oxygene gas. When the bottom of the retort is gently heated by a spirit lamp, it throws off most vivid scintillations like those from the combustion of the bark of charcoal, and the mass burns with a brilliant light. A sublimate rises from it, which is boracic acid; and it becomes coated with a vitreous substance, which proves likewise to be boracic acid; and after this has been washed off, the residuum appears perfectly black, and requires a higher temperature for its inflammation than the olive coloured substance; and by its inflammation produces a fresh portion of boracic acid.

In oxymuriatic acid gas, the peculiar inflammable substance occasions some beautiful phenomena. When this gas is brought in contact with it at common temperatures, it instantly takes fire and burns with a brilliant white light, a white substance coats the interior of the vessel in which the experiment is made, and the peculiar substance is found covered by a white film, which by washing affords boracic acid, and leaves a black matter, which is not spontaneously inflammable in a fresh portion of the gas; but which inflames in it by a gentle heat, and produces boracic acid.

The peculiar inflammable substance, when heated nearly to redness in hydrogene, or nitrogene, did not seem to dissolve in these gasses, or to act upon them; it merely gained a darker shade of colour, and a little moisture rose from it, which condensed in the neck of the retort in which the experiment was made.

On the fluid menstrua containing oxygene, it produced effects which might be looked for from the phenomena of its agency on gasses.

When thrown into concentrated nitric acid, it rendered it bright red, so that nitrous gas was produced and absorbed, but it did not dissolve rapidly till the acid was heated; when there was a considerable effervescence, the peculiar substance disappeared, nitrous gas was evolved, and the fluid afforded boracic acid.

It did not act upon concentrated sulphuric acid till heat was applied; it then produced a slight effervescence; the acid became black at its points of contact with the solid; and a deep brown solution was formed, which, when neutralized by potash, gave a black precipitate.

When heated in a strong solution of muriatic acid, it gave it a faint tint of green; but there was no vividness of action, or considerable solution.

On acetic acid heated, it had no perceptible action.

It combined with the fixed alkalies, both by fusion and aqueous solution, and formed pale olive coloured compounds, which gave dark precipitates when decomposed by muriatic acid.

When it was kept long in contact with sulphur in fusion, it slowly dissolved, and the sulphur acquired an olive tint. It

was still less acted upon by phosphorus, and after an hour's exposure to it, had scarcely diminished in quantity, but the phosphorus had gained a tint of pale green.

It did not combine with mercury, when they were heated together.

These circumstances are sufficient to shew, that the combustible substance obtained from boracic acid by the agency of potassium, is different from any other known species of matter, and it seems, as far as the evidence extends, to be the same as that procured from it by electricity; and the two series of facts, seem fully to establish the decomposition, and recomposition of the acid.

From the large quantity of potassium required to decompose a small quantity of the acid, it is evident that the boracic acid must contain a considerable proportion of oxygene, I have endeavoured to determine the relative weights of the peculiar inflammable matter and oxygene, which compose a given weight of boracic acid; and to this end I made several analytical and synthetical experiments; I shall give the results of the two which I consider as most accurate.

Twenty grains of boracic acid and thirty grains of potassium, were made to act upon each other by heat in a tube of brass; the result did not effervesce when washed with diluted muriatic acid; and there were obtained after the process, by alight lixiviation in warm water, two grains and about $\frac{4}{10}$ of the olive coloured matter. Now thirty grains of potassium, would require about five grains of oxygene, to form thirty-five of potash; and according to this estimation, boracic acid must consist of about one of the peculiar inflammable substance, to nearly two of oxygene.

A grain of the inflammable substance in very fine powder, and diffused over a large surface, was set fire to in a retort, containing twelve cubical inches of oxygene; three cubical inches of gas were absorbed, and the black residuum collected after the boracic acid had been dissolved, was found to equal five eighths of a grain. This, by a second combustion, was almost entirely converted into boracic acid, with the absorption of two cubical inches and one eighth more of oxygene. The thermometer in this experiment was at 58° FAHRENHEIT, and the barometer at 30.2.

According to this result, boracic acid would consist of one of the inflammable matter, to about 1.8 of oxygene; and the dark residual substance, supposing it to be simply the inflammable matter combined with less oxygene than is sufficient to constitute boracic acid, would be an oxide, consisting of about 4.7 of inflammable matter, to 1.55 of oxygene.

These estimations, I do not however venture to give, as entirely correct. In the analytical experiments, there are probably sources of error, from the solution of a part of the inflammable matter, and it possibly may retain alkali, which cannot be separated by the acid. In the synthetical process, in which washing is employed, and so small a quantity of matter used, the results are still less to be depended upon; they must be considered only as imperfect approximations.

From the general tenour of the facts, it appears that the combustible matter obtained from boracic acid, bears the same relation to that substance, as sulphur and phosphorus do to the sulphuric and phosphoric acids. But is it an elementary

inflammable body, the pure basis of the acid? or is it not like sulphur and phosphorus, compounded?

Without entering into any discussion concerning ultimate elementary matter, there are many circumstances which favour the idea, that the dark olive substance, is not a simple body: its being non-conducting, its change of colour by being heated in hydrogene gas, and its power of combining with the alkalies; for these properties in general belong to primary compounds, that are known to contain oxygene.

I heated the olive coloured substance with potassium, there was a combination, but without any luminous appearance, and a gray metallic mass was formed; but from the effect of this upon water, I could not affirm that any oxygene had been added to the metal, the gas given off had a peculiar smell, and took up more oxygene by detonation than pure hydrogene, from which it seems probable, that it held some of the combustible matter in solution.

It occurred to me, that if the pure inflammable basis were capable of being deoxygenated by potassium, it would probably possess a stronger affinity for oxygene, than hydrogene, and therefore be again brought to its former state by water. I made another experiment on the operation of potassium, on the olive coloured substance, and exposed the mixture to a small quantity of ether, hoping that this might contain only water enough to oxygenate the potassium; but the same result occurred as in the last case; and a combination of potash and the olive coloured substance was produced, insoluble in ether.

I covered a small globule of potassium, with four or five times its weight of the olive coloured matter, in a platina tube

exhausted, after being filled with hydrogene; and heated the mixture to whiteness: no gas was evolved. When the tube was cooled, naphtha was poured into it, and the result examined under naphtha. Its colour was of a dense black. It had a lustre scarcely inferior to that of plumbago. It was a conductor of electricity. A portion of it thrown into water, occasioned a slight effervescence; and the solid matter separated, appeared dark olive, and the water became slightly alkaline. Another portion examined, after being exposed to air for a few minutes, had lost its conducting power, was brown on the surface, and no longer produced an effervescence in water.

Some of the olive inflammable matter, with a little potassium, was heated to whiteness, covered with iron filings, a dark metalline mass was formed, which conducted electricity, and which produced a very slight effervescence in water, and gave by solution in nitric acid, oxide of iron and boracic acid.

The substance which enters into alloy with potassium, and with iron, I am inclined to consider, as the true basis of the boracic acid.

In the olive coloured matter, this basis seems to exist in union with a little oxygene; and when the olive coloured substance is dried at common temperatures, it likewise contains water.

In the black non-conducting matter, produced in the combustion of the olive coloured substance, the basis is evidently combined with much more oxygene, and in its full state of oxygenation, it consitutes boracic acid.

From the colour of the oxides, their solubility in alkalies,

and from their general powers of combination, and from the conducting nature and lustre of the matter produced by the action of a small quantity of potassium upon the olive coloured substance, and from all analogy; there is strong reason to consider the boracic basis as metallic in its nature, and I venture to propose for it the name of *boracium*.

7. *Analytical Inquiries respecting Fluoric Acid.*

I have already laid before the Society, the account of my first experiments on the action of potassium, on fluoric acid gas.*

I stated, that the metal burns when heated in this elastic fluid, and that there is a great absorption of the gas.

Since the time that this communication was made, I have carried on various processes, with the view of ascertaining, accurately, the products of combustion, and I shall now describe their results.

When fluoric acid gas, that has been procured in contact with glass, is introduced into a plate glass retort, exhausted after being filled with hydrogene gas, white fumes are immediately perceived. The metal loses its splendour, and becomes covered with a grayish crust.

When the bottom of the retort is gently heated, the fumes become more copious; they continue for some time to be emitted, but at last cease altogether.

* Phil. Trans. Part II. 1808, p. 343. The combustion of potassium in fluoric acid, I have since seen mentioned in the number of the Moniteur, already so often quoted, as observed by M. M. Gay Lussac and Thenard; but no notice is taken of the results.

If the gas is examined at this time, its volume is found to be a little increased, by the addition of a small quantity of hydrogene.

No new fumes are produced by a second application of a low heat; but when the temperature is raised nearly to the point of sublimation of potassium, the metal rises through the crust, becomes first of a copper colour and then of a bluish black, and soon after inflames and burns with a most brilliant red light.

After this combustion, either the whole or a part of the fluoric acid, according as the quantity of potassium is great or small, is found to be destroyed or absorbed. A mass of a chocolate colour remains in the bottom of the retort; and a sublimate, in some parts chocolate, and in others yellow, is found round the sides, and at the top of the retort.

When the residual gas afforded by this operation, is washed with water, and exposed to the action of an electrical spark mixed with oxygene gas, it detonates and affords a diminution, such as might be expected from hydrogene gas.

The proportional quantity of this elastic fluid, differs a little in different operations. When the fluoric acid has not been artificially dried, it amounts to one sixth or one seventh of the volume of the acid gas used; but when the fluoric acid has been long exposed to calcined sulphate of soda, it seldom amounts to one tenth.

I have endeavoured to collect large quantities of the chocolate coloured substance for minute examination; but some difficulties occurred.

When I used from eighteen to twenty grains of potassium, in a retort containing from twenty to thirty cubical inches of

fluoric acid gas, the intensity of the heat was such, as to fuse the bottom of the retort, and destroy the results.

In a very thick plate glass retort, containing about nineteen cubical inches of gas, I once succeeded in making a decisive experiment on ten grains and a half of potassium, and I found that about fourteen cubical inches of fluoric acid disappeared, and about two and a quarter of hydrogene gas were evolved. The barometer stood at 30.3, and the thermometer at 61° FAHRENHEIT; the gas had not been artifically dried. In this experiment there was very little sublimate; but the whole of the bottom of the retort was covered with a brown crust, and near the point of contact with the bottom, the substance was darker coloured, and approaching in its tint to black.

When the product was examined by a magnifier, it evidently appeared consisting of different kinds of matter; a blackish substance, a white, apparently saline substance, and a substance having different shades of brown and fawn colour.

The mass did not conduct electricity, and none of its parts could be separated, so as to be examined as to this property.

When a portion of it was thrown into water, it effervesced violently, and the gas evolved had some resemblance in smell to phosphuretted hydrogene, and was inflammable.

When a part of the mass was heated in contact with air, it burnt slowly, lost its brown colour, and became a white saline mass.

When heated in oxygene gas, in a retort of plate glass, it absorbed a portion of oxygene, but burnt with difficulty, and required to be heated nearly to redness; and the light given

out was similar to that produced by the combustion of liver of sulphur.

The water which had acted upon a portion of it was examined; a number of chocolate coloured particles floated in it. When the solid matter was separated by the filter, the fluid was found to contain fluate of potash, and potash. The solid residuum was heated in a small glass retort in oxygene gas; it burnt before it had attained a red heat, and became white. In this process, oxygene was absorbed, and acid matter produced. The remainder possessed the properties of the substance formed from fluoric acid gas holding siliceous earth in solution, by the action of water.

In experiments made upon the combustion of quantities of potassium equal to from six to eleven grains, the portion of matter separable from the water has amounted to a very small part of a grain only, and operating upon so minute a scale, I have not been able to gain fully decided evidence, that the inflammable part of it is the pure basis of the fluoric acid; but with respect to the decomposition of this body by potassium, and the existence of its basis at least combined with a smaller proportion of oxygene in the solid product generated, and the regeneration of the acid by the ignition of this product in oxygene gas, it is scarcely possible to entertain a doubt.

The decomposition of the fluoric acid by potassium, seems analogous to that of the acids of sulphur and phosphorus. In neither of these cases are the pure bases, or even the bases in their common form evolved; but new compounds result, and in one case sulphurets, and sulphites, and in the other phosphurets, and phosphites of potash, are generated.

As silex was always obtained during the combustion of the

chocolate coloured substance obtained by lixiviation, it occurred to me that this matter might be a result of the operation, and that the chocolate substance might be a compound of the siliceous and fluoric bases in a low state of oxygenation, with potash; and this idea is favoured by some trials that I made to separate silex from the mass, by boiling it in concentrated fluoric acid; the substance did not seem to be much altered by this process, and still gave silex by combustion.

I endeavoured to decompose fluoric acid gas in a perfectly dry state, and which contained no siliceous earth; and for this purpose I made a mixture of one hundred grains of dry boracic acid, and two hundred grains of fluor spar, and placed them in the bottom of an iron tube, having a stop-cock and a tube of safety attached to it.

The tube was inserted horizontally in a forge, and twenty grains of potassium, in a proper iron tray, introduced into that part of it where the heat was only suffered to rise to dull redness. The bottom of the tube was heated to whiteness, and the acid acted upon by the heated potassium, as it was generated. After the process was finished, the result in the tray was examined.

It was in some parts black, and in others of a dark brown. It did not effervesce with water: and when lixiviated, afforded a dark brown combustible mass, which did not conduct electricity, and which when burnt in oxygene gas, afforded boracic, and fluoric acid. It dissolved with violent effervescence in nitric acid; but did not inflame spontaneously in oxymuriatic acid gas.

I have not as yet examined any of the other properties of

this substance; but I am inclined to consider it as a compound of the olive coloured oxide of boracium, and an oxide of the fluoric basis.

In examining the dry fluoric acid gas, procured in a process similar to that which has been just described, it gave very evident marks of the presence of boracic acid.

As the chocolate coloured substance is permanent in water, it occurred to me that it might possibly be producible from concentrated liquid fluoric acid at the negative surface in the VOLTAIC circuit.

I made the experiment with platina surfaces, from a battery of two hundred and fifty plates of six inches, on fluoric acid the densest that could be obtained by the distillation of fluor spar and concentrated sulphuric acid of commerce, in vessels of lead. Oxygene and hydrogene were evolved, and a dark brown matter separated at the deoxydating surface; but the result of an operation conducted for many hours, merely enabled me to ascertain that it was combustible, and produced acid matter in combustion; but I cannot venture to draw the conclusion that this acid was fluoric acid, as it was not impossible that some sulphureous, or sulphuric acid might likewise exist in the solution.

I heated the olive coloured inflammable substance, obtained from the boracic acid, in common fluoric acid gas in a plate glass retort; the temperature was raised till the glass began to fuse; but no change, indicating a decomposition, took place.

I heated six grains of potassium with four grains of powdered fluor spar in a green glass tube filled with hydrogene; there was a slight ignition, a minute quantity of hydrogene

gas was evolved, and a dark gray mass was produced, which acted upon water with much effervescence, but left no solid inflammable residuum.

8. *Analytical Experiments on Muriatic Acid.*

I have made a greater number of experiments upon this substance, than upon any of the other subjects of research that have been mentioned; it will be impossible to give any more than a general view of them within the limits of the Bakerian lecture.

Researches carried on some years ago, and which are detailed in the Journals of the Royal Institution, shewed that there were little hopes of decomposing muriatic acid, in its common form, by VOLTAIC electricity. When aqueous solution of muriatic acid is acted upon, the water alone is decomposed; and the VOLTAIC electrization of the gas affords no indications of its decomposition; and merely seems to shew, that this elastic fluid contains much more water than has been usually suspected.

I have already laid before the Society, an account of some experiments made on the action of potassium on muriatic acid. I have since carried on the same processes on a larger scale, but with precisely similar results.

When potassium is introduced into muriatic acid gas, procured from muriate of ammonia and concentrated sulphuric acid, and freed from as much moisture as muriate of lime is capable of attracting from it, it immediately becomes covered with a white crust, it heats spontaneously, and by the assistance of a lamp, acquires in some parts the temperature of ignition, but does not inflame. When the potassium and the gas are in

proper proportions, they both entirely disappear; a white salt is formed, and a quantity of pure hydrogene gas evolved, which equals about one third of the original volume of the gas.

By eight grains of potassium employed in this way, I effected the absorption of nearly twenty two cubical inches of muriatic acid gas; and the quantity of hydrogene gas produced was equal to more than eight cubical inches.

The correspondence between the quantity of hydrogene generated in cases of this kind, and by the action of potassium upon water, combined with the effects of ignited charcoal upon muriatic acid gas, by which a quantity of inflammable gas is produced equal to more than one third of its volume; seemed to shew, that the phenomena merely depended upon moisture combined with the muriatic acid gas.*

To determine this point with more certainty however, and to ascertain whether or no the appearance of the hydrogene was wholly unconnected with the decomposition of the acid, I made two comparative experiments on the quantity of muriate of silver, furnished by two equal quantities of muriatic acid, one of which had been converted into muriate of potash by the action of potassium, and the other of which had been absorbed by water; every care was taken to avoid sources of error; and it was found that there was no notable difference in the weight of the results.

* When the VOLTAIC spark is taken continuously, by means of points of charcoal in muriatic acid gas over mercury, muriate of mercury is rapidly formed, a volume of inflammable gas, equal to one third of the original volume of the muriatic acid gas appears. The acid gas enters into combination with the oxide of mercury, as that water enough is present in the experiment to form oxide sufficient to absorb the whole of the acid.

There was no proof then, that the muriatic acid had been decompounded in these experiments; and there was every reason to consider it as containing in its common aeriform state, at least one third of its weight of water; and this conclusion we shall find warranted by facts, which are immediately to follow.

I now made a number of experiments, with the hopes of obtaining the muriatic acid free from water.

I first heated to whiteness, in a well luted porcelain retort, a mixture of dry sulphate of iron, and muriate of lime which had been previously ignited; but a few cubic inches of gas only, were obtained, though the mixture was in the quantity of several ounces; and this gas contained sulphureous acid. I heated dry muriate of lime, mixed both with phosphoric glass and dry boracic acid, in tubes of porcelain, and of iron, and employed the blast of an excellent forge; but by neither of these methods was any gas obtained, though when a little moisture was added to the mixtures, muriatic acid was develloped in such quantities, as almost to produce explosions.

The fuming muriate of tin, *the liquor of Libavius*, is known to contain dry muriatic acid. I attempted to separate the acid from this substance, by distilling it with sulphur and with phosphorus; but without success. I obtained only triple compounds, in physical characters, something like the solutions of phosphorus, and sulphur in oil, which were non-conductors of electricity, which did not redden dry litmus paper, and which evolved muriatic acid gas with great violence, heat, and ebullition on the contact of water.

I distilled mixtures of corrosive sublimate and sulphur, and of calomel and sulphur; when these were used in their

common states, muriatic acid gas was evolved; but when they were dried by a gentle heat, the quantity was exceedingly diminished, and the little gas that was generated gave hydrogene by the action of potassium. During the distillation of corrosive sublimate and sulphur, a very small quantity of a limpid fluid passed over. When examined by transmitted light, it appeared yellowish green. It emitted fumes of muriatic acid, did not redden dry litmus paper, and deposited sulphur by the action of water. I am inclined to consider it as a modification of the substance discovered by Dr. THOMSON, in his experiments on the action of oxymuriatic acid on sulphur.

M. M. GAY LUSSAC and THENARD * have mentioned, that they endeavoured to procure dry muriatic acid by distilling a mixture of calomel and phosphorus, and that they obtained a fluid which they consider as a compound of muriatic acid, phosphorus, and oxygene. In distilling corrosive sublimate with phosphorus, I had a similar result, and I obtained the substance in much larger quantities, than by the distillation of phosphorus with calomel.

As oxymuriatic acid is slightly soluble in water, there was reason to suppose, reciprocally that water must be slightly soluble in this gas; I endeavoured therefore to procure dry muriatic acid, by absorbing the oxygene from oxymuriatic acid gas by substances, which when oxygenated, produce compounds possessing a strong affinity for water. Phosphorus, it is well known, burns in oxymuriatic acid gas; though the results of this combustion, I believe, have never been minutely examined. With the hopes of procuring muriatic acid gas, free from moisture, I made the experiment.

* The Moniteur before quoted.

I introduced phosphorus into a receiver having a stop-cock, which had been exhausted, and admitted oxymuriatic acid gas. As soon as the retort was full, the phosphorus entered into combustion, throwing forth pale white flames. A white sublimate collected in the top of the retort, and a fluid as limpid as water, trickled down the sides of the neck. The gas seemed to be entirely absorbed, for when the stop-cock was opened, a fresh quantity of oxymuriatic acid, nearly as much as would have filled the retort, entered.

The same phenomenon of inflammation again took place, with similar results. Oxymuriatic acid gas was admitted till the whole of the phosphorus was consumed.

Minute experiments proved, that no gaseous muriatic acid had been evolved in this operation, and the muriatic acid was consequently to be looked for either in the white sublimate, or in the fluid which had formed in the neck of the retort.

The sublimate was in large portions, the fluid only in the quantity of a few drops. I collected by different processes, sufficient of both for examination.

The sublimate emitted fumes of muriatic acid when exposed to air. When brought in contact with water, it evolved muriatic acid gas, and left phosphoric acid, and muriatic acid, dissolved in the water. It was a non-conductor of electricity, and did not burn when heated; but sublimed when its temperature was about that of boiling water, leaving not the slightest residuum. I am inclined to regard it as a combination of phosphoric, and muriatic acid in their dry states.

The fluid was of a pale greenish yellow tint, and very limpid; when exposed to air, it rapidly disappeared, emitting dense

white fumes which had a strong smell differing a little from that of muriatic acid.

It reddened litmus paper in its common state, but had no effect upon litmus paper which had been well dried, and which was immediately dipped into it. It was a non-conductor of electricity. It heated when mixed with water, and evolved muriatic acid gas. I consider it as a compound of phosphorous acid, and muriatic acid, both free from water.*

Having failed in obtaining uncombined muriatic acid in this way, I performed a similar process with sulphur, but I was unable to cause it to inflame in oxymuriatic acid gas. When it was heated in it, it produced an orange coloured liquid, and yellow fumes passed into the neck of the retort, which condensed into a greenish yellow fluid. By repeatedly passing oxymuriatic acid through this fluid, and distilling it several times in the gas, I rendered it of a bright olive colour, and in this case it seemed to be a compound of dry sulphuric, and muriatic acid, holding in solution a very little sulphur. When it was heated in contact with sulphur, it rapidly dissolved it, and then became of a bright red colour, and when saturated with sulphur, of a pale golden colour.† No permanent aeriform fluid was evolved in any of these operations, and no muriatic gas appeared, unless moisture was introduced.

As there seemed little chance of procuring uncombined

* I attempted to obtain dry muriatic acid likewise from the phosphuretted muriatic acid of M. M. GAY LUSSAC and THENARD, by distilling it in retorts containing oxygene gas, and oxymuriatic acid gas. In the first case, the retort was shattered by the combustion of the phosphorus, with a violent explosion. In the second, compounds, similar to those described above, were formed.

† All these substances seem to be of the same nature as the singular compound, the sulphuretted muriatic acid, discovered by Dr. THOMSON, noticed in page 93.

muriatic acid, it was desirable to ascertain what would be the effects of potassium upon it in these singular compounds.

When potassium was introduced into the fluid, generated by the action of phosphorus on corrosive sublimate, at first it slightly effervesced, from the action of the liquid on the moist crust of potash surrounding it; but the metal soon appeared perfectly splendid, and swimming on the surface. I attempted to fuse it by heating the fluid, but it entered into ebullition at a temperature below that of the fusion of the potassium; indeed the mere heat of the hand was sufficient for the effect. On examining the potassium, I found that it was combined at the surface with phosphorus, and gave phosphuretted hydrogene by its operation upon water.

I endeavoured, by repeatedly distilling the fluid from potassium in a close vessel, to free it from phosphorus, and in this way I succeeded in depriving it of a considerable quantity of this substance.

I introduced ten or twelve drops of the liquid, which had been thus treated, into a small plate glass retort, containing six grains of potassium; the retort was exhausted after having been twice filled with hydrogene, the liquid was made to boil, and the retort kept warm till the whole had disappeared as elastic vapour. The potassium was then heated by the point of a spirit lamp; it had scarcely melted, when it burst into a most brilliant flame, as splendid as that of phosphorus in oxygene gas, and the retort was destroyed by the rapidity of combustion.

In other trials made upon smaller quantities after various failures, I was at last able to obtain the results; there was no proof of the evolution of any permanent elastic fluid during

the operation. A solid mass remained of a greenish colour at the surface, but dark gray in the interior. It was extremely inflammable, and often burnt spontaneously when exposed to air; when thrown upon water, it produced a violent explosion, with a smell like that of phosphuretted hydrogene. In the residuum of its combustion there was found muriate of potash, and phosphate of potash.

I endeavoured to perform this experiment in an iron tube, hoping that if the muriatic acid was decomposed in the process, its inflammable element, potassium and phosphorus, might be separated from each other by a high degree of heat; but in the first part of the operation the action was so intense, as to produce a destruction of the apparatus, and the stop-cock was separated from the tube with a loud detonation.

I heated potassium in the vapour of the compound of muriatic and phosphoric acid; but in this case, the inflammation was still more intense, and in all the experiments that I have hitherto tried, the glass vessels have been either fused or broken; the solid residuum has however appeared to be of the same kind as that I have just described.

The results of the operation of the sulphuretted compounds containing muriatic acid free from water upon potassium, are still more extraordinary than those of the phosphuretted compounds.

When a piece of potassium is introduced into the substance that distils over during the action of heated sulphur upon oxymuriatic acid, it at first produces a slight effervescence, and if the volume of the potassium considerably exceeds that of the liquid, it soon explodes with a violent report, and a most intense light.

I have endeavoured to collect the results of this operation, by causing the explosion to take place in large exhausted plate glass retorts; but, except in a case in which I used only about a quarter of a grain, I never succeeded. Generally the retort, though connected with the air pump at the time, was broken into atoms; and the explosion produced by a grain of potassium, and an equal quantity of the fluid, has appeared to me considerably louder than that of a musket.

In the case in which I succeeded in exploding a quarter of a grain, it was not possible for me to ascertain if any gaseous matter was evolved; but a solid compound was formed of a very deep gray tint, which burnt, throwing off bright scintillations, when gently heated, which inflamed when touched with water, and gave most brilliant sparks, like those thrown off by iron in oxygene gas.

Its properties certainly differed from those of any compound of sulphur and potassium that I have seen: whether it contains the muriatic basis must however be still a matter of inquiry.

There is, however, much reason for supposing, that in the singular phenomena of inflammation and detonation that have been described, the muriatic acid cannot be entirely passive: and it does not seem unfair to infer, that the transfer of its oxygene and the production of a novel substance, are connected with such effects, and that the highly inflammable nature of the new compounds, partly depends upon this circumstance. I am still pursuing the inquiry, and I shall not fail immediately to communicate to the Society, such results as may appear to me worthy of their attention.

9. *Some general Observations, with Experiments.*

An experiment has been lately published, which appeared so immediately connected with the discussion entered into in the second section of this Paper, that I repeated it with much earnestness.

In Mr. NICHOLSON's Journal for December, Dr. WOODHOUSE has given an account of a process, in which the action of water caused the inflammation of a mixture of four parts of charcoal and one of pearlash that had been strongly ignited together, and the emission of ammonia from them. I thought it possible, that in this case a substance might be formed similar to the residuum described in page 50; but by cooling the mixture out of the contact of nitrogene, I found that no ammonia was formed; and this substance evidently owed its existence to the absorption of atmospherical air by the charcoal.*

The experiments that I have detailed on the acids, offer some new views with respect to the nature of acidity. That a compound of muriatic acid with oxide of tin or phosphorus should

* Potash or pearlash is easily decomposed by the combined attractions of charcoal and iron; but it is not decomposable by charcoal, or, when perfectly dry, by iron alone. Two combustible bodies seem to be required by their combined affinities for the effect; thus in the experiment with the gun barrel, iron and hydrogene are concerned. I consider HOMBERG's pyrophorus as a triple compound of potassium, sulphur, and charcoal; and in this ancient process, the potash is probably decomposed by two affinities. The substance is perfectly imitated by heating together ten parts of charcoal, two of potassium, and one of sulphur.

When I first shewed the production of potassium to Dr. WOLLASTON in October 1807, he stated, that this new fact induced him to conceive that the action of potash upon platina, was owing to the formation of potassium, and proposed it, as a matter of research, whether the alkali might not be decomposed by the joint action of platina and charcoal.

not reddened vegetable blues, might be ascribed to a species of neutralization, by the oxide or inflammable body; but the same reasoning will not apply to the dry compounds which contain acid matter only, and which are precisely similar as to this quality. Let a piece of dry and warm litmus paper be moistened with the compound of muriatic and phosphorous acid, it perfectly retains its colour. Let it then be placed upon a piece of moistened litmus paper, it instantly becomes of a bright red, heats and develloped muriatic acid gas.

All the fluid acids that contain water are excellent conductors of electricity, in the class called that of imperfect conductors; but the compounds to which I have just alluded, are non-conductors in the same degree as oils, with which they are perfectly miscible. When I first examined muriatic acid, in its combinations free from moisture, I had great hopes of decomposing them by electricity; but there was no action without contact of the wires, and the spark seemed to separate no one of their constituents, but only to render them gaseous. The circumstance likewise applies to the boracic acid, which is a good conductor as long as it contains water; but which, when freed from water and made fluid by heat, is then a non-conductor.

The alkalies and the earthy compounds, and the oxides, as dry as we can obtain them, though non-conductors when solid, are, on the contrary, all conductors when rendered fluid by heat.

When muriatic acid, existing in combination with phosphorous or phosphoric acid, is rendered gaseous by the action of water, the quantity of this fluid that disappears, at least equals from one third to two fifths of the weight of the acid gas

produced; a circumstance that agrees with the indications given by the action of potassium.*

I attempted to procure a compound of dry muriatic and carbonic acids, hoping that it might be gaseous, and that the two acids might be decomposable at the same time by potassium. The process that I employed was by passing corrosive sublimate in vapour through charcoal ignited to whiteness; but I obtained a very small quantity of gas, which seemed to be a mixture of common muriatic acid gas and carbonic acid gas; a very minute portion of running mercury only was obtained, by a long continuation of the process; and the slight decomposition that did take place, I am inclined to attribute to the production of water, by the action of the hydrogene of the charcoal upon the oxygene of the oxide of mercury.†

In mixing muriatic acid gas with carbonic acid, or oxygene, or hydrogene, the gases being in their common states, as to moisture, there was always a cloudiness produced; doubtless owing to the attraction of their water to form liquid muriatic acid.

On fluoric acid gas no such effect was occasioned. This fact, at first view, might be supposed to shew, that the hydrogene evolved by the action of potassium upon fluoric acid gas, is

* Page 98.

† These facts and the other facts of the same kind, explain the difficulty of the decomposition of the metallic muriates in common processes of metallurgy. They likewise explain other phenomena in the agencies of muriatic salts. In all cases when a muriatic salt is decomposed by an acid, and muriatic acid gas set free, there appears to be a double affinity, that of the acid for the basis, and of the muriatic acid for water; pure muriatic acid does not seem capable of being displaced by any other acid.

owing to water in actual combination with it, like that in muriatic acid gas, and which may be essential to its elastic state; but it is more probable, from the smallness of the quantity, and from the difference of the quantity in different cases, that the moisture is merely in that state of diffusion or solution in which it exists in gases in general, though from the disposition of water to be deposited in this acid gas in the form of an acid solution, it must be either less in quantity, or in a less free state, so as to require for its exhibition much more delicate hygrometrical tests.

The facts advanced in this Lecture, afford no new arguments in favour of an idea to which I referred in my last communication to the Society, that of hydrogene being a common principle in all inflammable bodies; and except in instances which are still under investigation, and concerning which no precise conclusions can as yet be drawn, the generalization of LAVOISIER happily applies to the explanation of all the new phenomena.

In proportion as progress is made towards the knowledge of pure combustible bases, so in proportion is the number of metallic substances increased; and it is probable that sulphur and phosphorus, could they be perfectly deprived of oxygene, would belong to this class of bodies. Possibly their pure elementary matter may be procured by distillation, at a high heat, from metallic alloys, in which they have been acted upon by sodium or potassium. I hope soon to be able to try this experiment.

As our inquiries at present stand, the great general division of natural bodies is into matter which is, or may be supposed to be, metallic, and oxygene; but till the problem concerning

the nature of nitrogene is fully solved, all systematic arrangements made upon this idea must be regarded as premature.

EXPLANATION OF THE FIGURES.

Fig. 1. The retort of plate glass for heating potassium in gases.

Fig. 2. The tray of platina for receiving the potassium.

Fig. 3. The platina tube for receiving the tray in experiments of distillation.

Fig. 4. The apparatus for taking the VOLTAIC spark in sulphur and phosphorus.

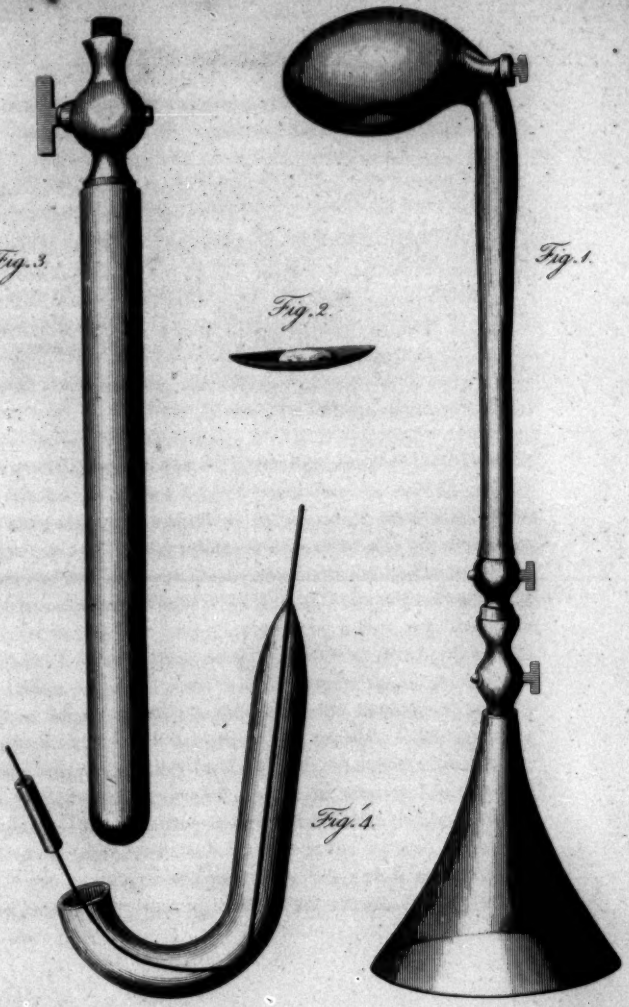
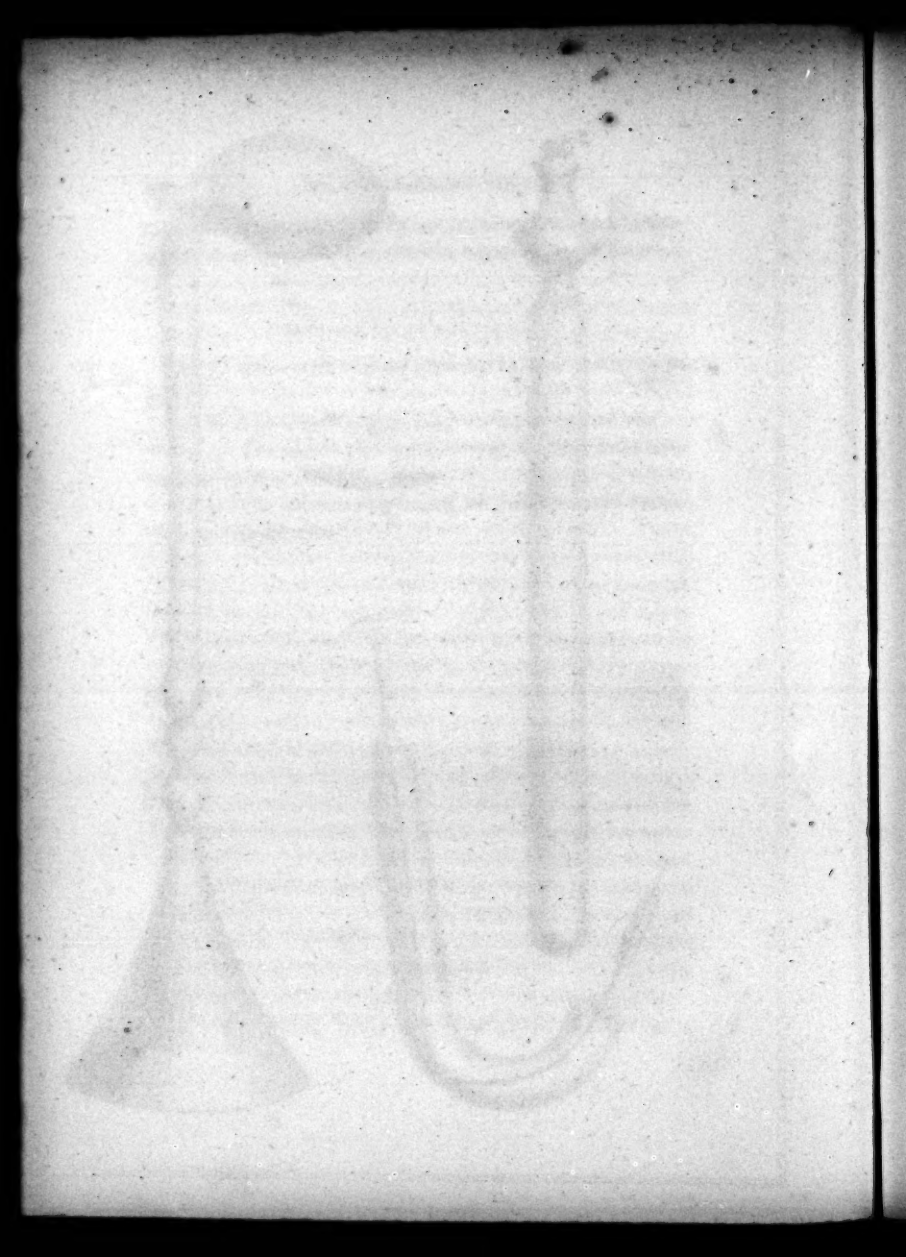


Fig. 3.
Fig. 1.
Fig. 2.
Fig. 4.



IV. *An Account of a Method of dividing Astronomical and other Instruments, by ocular Inspection; in which the usual Tools for graduating are not employed; the whole Operation being so contrived, that no Error can occur but what is chargeable to Vision, when assisted by the best optical Means of viewing and measuring minute Quantities.* By Mr. Edward Troughton. *Communicated by the Astronomer Royal.*

Read February 2, 1809.

It would ill become me, in addressing myself to the Members of this Society upon a subject which they are so well enabled to appreciate, to arrogate to myself more than may be assigned as my due, for whatever of success may have been the result of my long continued endeavours, exerted in prosecuting towards perfection *the dividing of Instruments immediately subservient to the purposes of Astronomy*. A man very naturally will set a value upon a thing on which so much of his life has been expended; and I shall readily, therefore, be pardoned for saying, that considering some attainments which I have made on this subject as too valuable to be lost, and being encouraged also by the degree of attention which the Royal Society has ever paid to practical subjects, I feel myself ambitious of presenting them to the public through what I deem the most respectable channel in the world.

It was as early as the year 1775, being then apprentice to my brother, the late Mr. John Troughton, that the art of

dividing had become interesting to me; the study of astronomy was also new and fascinating; and I then formed the resolution to aim at the nicer parts of my profession.

At the time alluded to, my brother, in the art of dividing, was justly considered the rival of RAMSDEN; but he was then almost unknown beyond the narrow circle of the mathematical and optical instrument makers; for whom he was chiefly occupied in the division, by hand, of small astronomical quadrants, and HADLEY's sextants of large radius. Notwithstanding my own employment at that time was of a much inferior nature, yet I closely inspected his work, and tried at leisure hours on waste materials to imitate it. With as steady a hand, and as good an eye, as young men generally have, I was much disappointed at finding, that after having made two points, neat and small to my liking, I could not bisect the distance between them, without enlarging, displacing, or deforming them with the points of the compasses. This circumstance gave me an early dislike to the tools then in use; and occasioned me the more uneasiness, as I foresaw that it was an evil which no practice, care, nor habit could entirely cure: Beam-compasses, spring-dividers, and a scale of equal parts, in short, appeared to me little better than so many sources of mischief.

I had already acquired a good share of dexterity, as a general workman: Of the different branches of our art, that of *turning* alone seemed to me to border on perfection. This juvenile conceit, fallacious as I afterwards found it, furnished the first train of thoughts, which led to the method about to be described; for it occurred to me, that if I could by any means apply the principle of turning to the art of dividing

instruments, the tools liable to objection might be dispensed with. The means of doing this was first suggested, by seeing the action of the perambulator, or measuring wheel; the surface of the earth presenting itself as the edge of the instrument to be divided, and the wheel of the perambulator as a narrow roller acting on that edge: and hence arose an idea that some easy contrivance might be devised, for marking off the revolutions and parts of the roller upon the instrument. Since the year above-mentioned, several persons have proposed to me, as new, dividing by the roller, and I have been told, that it also occurred long ago to Hooke, Sisson, and others; but, as Hatton on Watch-making, says, "I do not "consider the man an inventor, who merely thinks of a thing; "to be an inventor, in my opinion, he must act successfully "upon the thought, so as to make it useful." I had no occasion, however, to have made an apology for acting upon a thought, which, unknown to me, had been previously conceived by others; for it will be seen in the sequel, how little the roller has to do in the result, and with what extreme caution it is found necessary to employ it.

When a roller is properly proportioned to the radius of the circle to be divided, and with its edge made a small matter conical, so that one side may be too great, and the other side too little, it may be adjusted so exactly, that it may be carried several times around the circle, without the error of a single second; and it acts with so much steadiness, that it may not unaptly be considered as a wheel and pinion of indefinitely high numbers. Yet, such is the imperfection of the edges of the circle and roller, that, when worked with the greatest care, the intermediate parts, on a radius of two feet, will sometimes be

unequal to the value of half a minute or more. After having found the terminating point of a quadrant or circle so permanent, although I was not prepared to expect perfect equality throughout, yet I was much mortified to find the errors so great, at least ten times as much as I expected; which fact indicated, beyond a doubt, that if the roller is to be trusted at all, it must only be trusted through a very short arc. Had there been any thing slippery in the action, which would have been indicated by measuring the same part at different times differently, there would have been an end of it at once; but, that not being the case in any sensible degree, the roller becomes an useful auxiliary to fill up short intervals, whose limits have been corrected by more certain means.*

* There are two things in the foregoing account of the action of the roller which have a tendency to excite surprise. The first is, that the roller should, in different parts of its journey round the circle, measure the latter so differently. One would not wonder, however, if in taking the measure across a ploughed field, it should be found different to a parallel measure taken upon a gravel-walk; and, in my opinion, the cases are not very dissimilar. Porosity of the metal, in one part of the circle more than in the other, must evidently have the same effect; brass unhammered is always porous; and the part, which has felt the effect of two blows, cannot be so dense as other parts which have felt the effect of three; and, should the edge of the circle be indented by *jarring-turning*, it would produce a visible similitude to ploughed ground: Every workman must be sufficiently upon his guard against such a palpable source of error; yet, perhaps with our greatest care we may not be able to avoid it altogether. The second is, that notwithstanding the inequality above-mentioned, the roller having reached the point upon the circle from whence it set out, should perform a second, third, &c. course of revolutions, without any sensible deviation from its former track; this is not perhaps so easily accounted for. It must be mentioned, that the exterior border of the circle should be *turned rounding*, presenting to the roller a convex edge, whose radius of curvature is not greater than one tenth of an inch. Now, were the materials perfectly inelastic and impenetrable, the roller could only touch the circle in a *point*, and in passing round the circle, it could only occupy a *line* of contact. This in practice is not the case; the circle always marks the roller with a

BIRD, who enjoyed the undisputed reputation of being the most accurate divider of the age in which he lived, was the first who contrived the means how to render the usual divisions of the quadrant bisectional; which property, except his being unusually careful in avoiding the effects of unequal expansion from change of temperature, chiefly distinguished his method from others who divided by hand. This desirable object he accomplished by the use which he made of a finely divided scale of equal parts. The thing aimed at was, to obtain a point upon the arc at the highest *bisectional number of divisions* from 0, which in his eight feet quadrants was 1024, $= 85° 20'$. The extent of the beam-compasses, with which he traced the arc upon the limb of the instrument to be divided, being set off upon that arc, gave the points 0° and 60°;

broad list, and thereby shews that there is a yielding between them to a considerable amount. The breadth of this list is not less than one fiftieth of an inch; and it follows, that at least 12° of the circle's edge must be in contact at the same time; that the two surfaces yield to each other in depth, by a quantity equal to the *ver. sin.* of half that arc, or $\frac{1}{1200}$ of an inch; and that the circle has always hold of the roller by nearly 1° of the edge of the latter. Whoever has examined the surfaces of metals which have rolled against each other, must have observed that peculiar kind of indentation that always accompanies their action; and there can be no doubt that the particles of a roller, and those of the surface on which it acts, which mutually indent each other, will, upon a second course begun from the same point, indent each other deeper: This is not, however, exactly the case in question; for, whatever of fitting might have taken place between the surfaces of our roller and circle in the first revolution of the former, one should imagine would be obliterated by the fifteen turns which it must repeat over fresh ground. Experience shows, however, as every one will find who tries the experiment with good work, that on coming round to the point of commencement, the roller has the disposition to regain its former track; for, were this not the case, although the commensurate diameters were adjusted so exactly as to be without sensible error in one course, yet a less error than that which is so would become visible, when repeated through many courses.

which, being bisected, gave 30° more to complete the total arc. A second order of bisections gave points at 15° distance from each other; but that which denoted 75° was most useful. Now, from the known length of the radius, as measured upon the scale, the length of the chord of 10° 20′ was computed, taken off from the scale, and protracted from 75° forwards; and the chord of 4° 40′, being ascertained in the same manner, was set off from 90° backwards, meeting the chord of 10° 20′ in the continually bisectional arc of 85° 20′. This point being found, the work was carried on by bisections, and the chords, as they became small enough, were set off beyond this point to supply the remainder of the quadrantal arc. My brother, whom I mentioned before, from mere want of a scale of equal parts upon which he could rely, contrived the means of dividing bisectionally without one. His method I will briefly state as follows, in the manner which it would apply to dividing a mural quadrant. The arcs of 60° and 30° give the total arc as before; and let the last arc of 30° be bisected, also the last arc of 15°, and again the last arc of 7° 30′: The two marks next 90° will now be 82° 30′ and 86° 15′, consequently the point sought lies between them. Bisections will serve us no longer; but if we divide this space equally into three parts, the most forward of the two intermediate marks will give us 85°, and if we divide the portion of the arc between this mark and 86° 15′ also into three, the most backward of the two marks will denote 85° 30′. Lastly, if we divide any one of these last spaces into five, and set off one of these fifth parts backwards from 85° 30′, we shall have the desired point at 1024 divisions upon the arc from 0°. All the rest of the divisions which have been made in this operation, which I have

called marks because they should be made as faint as possible, must be erased; for my brother would not suffer a mark to remain upon the arc to interfere with his future bisections.

Mr. SMEATON, in a paper to be more particularly noticed presently, justly remarks the want of a unity of principle in Mr. BIRD's method; for he proceeds partly on the ground of the protracted radius, and partly upon that of the computed chord; which, as SMEATON observes, may or may not agree. BIRD, without doubt, used the radius and its parts in order to secure an exact quadrant; but SMEATON, treating exactness in the total arc as of little value to astronomy, would, in order to secure the more essential property of equality of division, reject the radius altogether, and proceed entirely upon the simple principle of the computed chord. The means pursued by my brother, to reach the point which terminates the great bisectional arc, is the only part in which it differs from BIRD's method; and, I think it is without prejudice that I give it the preference. It is obvious that it is as well calculated to procure equality of division, as the means suggested by SMEATON; at the same time that it is equal to BIRD's in securing the precise measure of the total arc. It proceeds entirely upon the principle of the protracted chord of 60° and its subdivision; and the uncertainty, which is introduced into the work by the sparing use which is made of subdivision by 3 and 5, is, in my opinion, likely to be much exceeded by the errors of a divided scale,* and those of the hand and eye, in taking off the computed chords, and applying them to the arc of the instrument to be divided.

* That BIRD's scale was not without considerable errors, will be shewn towards the end of this paper.

RAMSDEN's well known method of dividing by the engine unites so much accuracy and facility, that a better can hardly be wished for; and I may venture to say that it will never be superseded, in the division of instruments of *moderate radii*. It was well suited to the time in which it appeared; a time when the improvements made in nautical astronomy, and the growing commerce of our country, called for a number of reflecting instruments, which never could have been supplied, had it been necessary to have divided them by hand; however, as it only applies to small instruments, it hardly comes within the subject of this paper.

The method of HINDLEY, as described by SMEATON,* I will venture to predict will never be put in practice for dividing astronomical instruments, however applicable it might formerly have been for obtaining numbers for cutting clock-work, for which purpose it was originally intended. It consists of a train of violent operations with blunt tools, any one of which is sufficient to stretch the materials beyond, or press them within their natural state of rest; and, although the whole is done by contact, the nature of this contact is such as, I think, ought rather to have been contrasted with, than represented as being similar to, the nature of the contact used in SMEATON's Pyrometer, which latter is performed by the most delicate touch; and is represented, I believe justly, to be sensible to the $\frac{1}{50000}$ part of an inch. SMEATON has, however, acquitted himself well, in describing and improving the method of his friend; and the world is particularly obliged to him for the historical part of his paper, as it contains valuable information which perhaps no one else could have written.

* Phil. Trans. for 1788.

The only method of dividing large instruments now practised in London, that I know of, besides my own, has not yet, I believe, been made public. It consists in dividing by hand with beam compasses and spring dividers, in the usual way; with the addition of examining the work by microscopes, and correcting it, as it proceeds, by pressing forwards or backwards by hand, with a fine conical point, those dots which appear erroneous; and thus adjusting them to their proper places. The method admits of considerable accuracy, provided the operator has a steady hand and good eye; but his work will ever be irregular and inelegant. He must have a circular line passing through the middle of his dots, to enable him to make and keep them at an equal distance from the centre. The bisectional arcs, also, which cut them across, deform them much; and, what is worse, the dots which require correction (about two thirds perhaps of the whole) will become larger than the rest, and unequally so in proportion to the number of attempts which have been found necessary to adjust them. In the course of which operation, some of them grow insufferably too large, and it becomes necessary to reduce them to an equality with their neighbours. This is done with the burnisher, and causes a hollow in the surface, which has a very disagreeable appearance. Moreover, dots which have been burnished up are always ill-defined, and of a bad figure. Sir GEORGE SHUCKBURGH EVELYN, in his paper on the Equatorial,* denominates these "doubtful or bad points;" and, (considering the few places which he examines) they bear no inconsiderable proportion to the whole. In my opinion, it would be a great improvement of this method, to

* Phil. Trans. for 1793.

divide the whole by hand at once, and afterwards to correct the whole; for a dot forced to its place, as above, will seldom allow the compass-point to rest in the centre of its apparent area; therefore other dots made from those will scarcely ever be found in their true places. This improvement also prevents the corrected dots from being injured, or moved, by the future application of the compasses, no such application being necessary.

I will now dismiss this method of dividing, with observing, that it is tedious in the extreme; and did I not know the contrary beyond a doubt, I should have supposed it to have surpassed the utmost limit of human patience.* When I made my first essay at subdividing with the roller, I used this method, according to the improvement suggested above, of correcting a few primitive points; but even this was too slow for one who had too much to do. Perhaps, however, had my instruments been divided for me by an assistant, I might not have grudged to have paid him for the labour of going through the whole work by the method of adjustment; nor have felt the necessity of contriving a better way.

I might now extend the account of my method of dividing to a great length; by relating the alterations which the

* At the time alluded to, the double microscopic micrometer was unknown to me, and I did not learn its use, for these purposes, till the year 1790, from General ROY's description of the large theodolite. Previous to that time, I had used a frame which carried a single wire very near the surface to be divided; this wire was moveable by a fine micrometer screw, and was viewed by a single lens inserted in the lower end of a tube, which, for the purpose of taking off the parallax, was 4 inches long. The greatest objection to this mode of constructing the apparatus is, that the wire being necessarily exposed, is apt to gather up the dust; yet it is preferable to the one now in use, in cases where any doubt is entertained of the accuracy of the plane which is to receive the divisions.

apparatus has undergone during a long course of years,* and the various manner of its application, before I brought it to its present state of improvement; but I think I may save myself that trouble, for truly I do not see its use: I will, therefore, proceed immediately to a disclosure of the method, as practised on a late occasion, in the dividing of a four feet meridian circle, now the property of STEPHEN GROOMBRIDGE, Esq. of Blackheath.

The surface of the circle which is to receive the divisions, as well as its inner and outer edges, but especially the latter, should be turned in the most exact and careful manner; the reason for which will be better understood, when we come to describe the mode of applying the roller: and, as no projection can be admitted beyond the limb, if the telescope, as is generally the case, be longer than the diameter, those parts which extend further must be so applied, that they may be removed during the operation of dividing. Fig. 1 and 2 represent the principal parts of the apparatus; Fig. 1 shewing the plan, and Fig. 2 the elevation; in both of which the same letters of reference are affixed to corresponding parts, and both are drawn to a scale of half dimensions. A A is a part of the circle, the surface of which is seen in the plan, and the edge is seen in the elevation. B B B is the main plate of the apparatus, resting with its four feet *a a a a* upon the surface of the arc; these feet, being screws, may be adjusted so as to take equal shares of the weight, and then are fastened by nuts below the plate,

* The full conception of the method had occupied my mind in the year 1778; but as my brother could not be readily persuaded to relinquish a branch of the business to me in which he himself excelled, it was not until September 1785 that I produced my first specimen, by dividing an astronomical quadrant of two feet radius.

as shewn in Fig. 2. C C and D D are two similar plates, each attached to the main plate, one above and the other below, by four pillars; and in them are centred the ends of the axis of the roller E. F and G are two friction wheels, the latter firmly fastened to B, but the former is fixed in an adjustable frame, by means of which adjustment these wheels and the roller E, may be made to press; the former on the interior, and the latter on the exterior edge of the circle, with an equal and convenient force.* At the extremities of the axis of the roller, and attached to the middle of the plates C and D, are two bridges, having a screw in each; by means of which an adjustment is procured for raising or lowering the roller respecting the edge of the circle, whereby the former, having its diameter at the upper edge about .001 of an inch greater than at the lower edge (being, as before described, a little conical), it may easily be brought to the position where it will measure the proper portion of the circle.

Much experience and thought upon the subject have taught me, that the roller should be equal to one sixteenth part of the circle to be divided, or that it should revolve once in 22° 30′; and that the roller itself should be divided into sixteen parts; no matter whether with absolute truth, for accuracy is not at all essential here. Each of such divisions of the roller will correspond with an angle upon the circle of 1° 24′ 22,″5, or $\frac{1}{256}$th part of the circle. This number of principal divisions was chosen, on account of its being capable of continual bisec-

* Sufficient spring for keeping the roller in close and uniform contact with the edge of the circle, is found in the apparatus, without any particular contrivance for that purpose: the bending of the pillars of the secondary frames and of the axis of the roller, chiefly supplies this property.

tion; but they do not fall in with the ultimate divisions of the circle, which are intended to be equal to 5′ each.

The next thing to be considered is, how to make the roller measure the circle. As two microscopes are here necessary, and those which I use are very simple, I will in this place give a description of them. Fig. 6 is a section of the full size, and sufficiently explains their construction, and the position of the glasses; but the micrometer part and manner of mounting it, are better shown at H, in Fig. 1 and 2. The micrometer part consists of an oblong square frame, which is soldered into a slit, cut at right angles in the main tube; another similar piece nicely fitted into the former, and having a small motion at right angles to the axis of the microscope, has at one end a cylindrical guide pin, and at the other a micrometer screw; a spring of steel wire is also applied, as seen in the section, to prevent play, by keeping the head of the micrometer in close contact with the fixed frame. This head is divided into one hundred parts, which are numbered each way to 50; the use of which will be shewn hereafter. A fine wire is stretched across the moveable frame, for the purpose of bisecting fine dots. Two of these microscopes are necessary; also a third, which need not have the divided head, and must have in the moveable frame two wires crossing each other at an angle of about 30°; this microscope is shewn at I, Fig. 1. In the two first micrometers, a division of the head is of the value of about 0″,2, and the power and distinctness such, that when great care is taken, a much greater error than to the amount of one of these divisions cannot well be committed in setting the wire across the image of a well made dot. The double eye-glass has a motion by hand, for producing distinct vision

of the wire; and distinct vision of the dots is procured by a similar adjustment of the whole microscope.

The first step towards sizing the roller, is to compute its diameter according to the measure of the circle, and to reduce it agreeably thereto, taking care to leave it a small matter too large. The second step is, after having brought the roller into its place in the Plate B B, to make a mark upon the surface of the circle near the edge, and a similar one upon the roller, exactly opposite each other; then carry the apparat is forward with a steady hand, until the roller has made sixteen revolutions: If, now, the mark upon the roller, by having over-reached the one upon the circle, shews it to be much too large, take it out of the frame and reduce it by turning accordingly: When by repeating this, it is found to be very near, it may be turned about .001 of an inch smaller on the lower edge, and so far its preparation is completed. The third and last step is, the use and adaptation of the two microscopes; one of these must take its position at H in Fig. 1, viewing a small well defined dot made for the purpose on the circle; the other, not represented in the figure, must also be fixed to the main plate of Fig. 1, as near to the former as possible, but viewing one of the divisions on the roller. With a due attention to each microscope, it will now be seen to the greatest exactness when, by raising or depressing the roller, its commensurate diameter is found.

Fig. 3 is a representation of the apparatus for transferring the divisions of the roller to the circle. It consists of two slender bars, which, being seen edgewise in the figure, have only the appearance of narrow lines; but, when looked at from above, they resemble the form of the letter A. They

are fastened to the main frame, as at W and Z, by short pillars, having also the off leg of the angle secured in the same manner; Y is a fine conical steel point for making the dots, and X is a feeler, whereby the point Y may be pressed down with a uniform force, which force may be adjusted, by bending the end of the bar just above the point, so as to make the dots of the proper size. The point Y yields most readily to a perpendicular action; but is amply secured against any eccentric or lateral deviation.

The apparatus, so far described, is complete for laying our foundation, *i. e.* making 256 primary dots; no matter whether with perfect truth, or not, as was said respecting the divisions of the roller; precision in either is not be expected, nor wished; but it is of some importance, that they should be all of the same size, concentric, small, and round. They should occupy a position very near the extreme border of the circle, as well to give them the greatest radius possible, as that there should be room for the stationary microscope and other mechanism, which will be described hereafter.

It must be noticed, that there is a clamp and adjusting screw attached to the main plate of Fig. 1; but, as it differs in no respect from the usual contrivances for quick and slow motion, it has been judged unnecessary to incumber the drawing with it.

Now, the roller having been adjusted, with one microscope H upon its proper dot on the circle, and the other microscope at the first division on the roller; place the apparatus of Fig. 3 so that the dotting point Y may stand directly over the place which is designed for the beginning of the divisions. In this position of things, let the feeler X be pressed down, until its

lower end comes in contact with the circle; this will carry down the point, and make the first impression, or primary dot, upon the circle; unclamp the apparatus, and carry it forwards by hand, until another division of the roller comes near the wire of the microscope; then clamp it, and with the screw motion make the coincidence complete; where again press upon the feeler for the second dot: proceed in this manner until the whole round is completed.

From these 256 erroneous divisions, by a certain course of examination, and by computation, to ascertain their absolute and individual errors, and to form these errors into convenient tables, is the next part of the process, and makes a very important branch of my method of dividing.

The apparatus must now be taken off, and the circle mounted in the same manner, that it will be in the Observatory. The two microscopes, which have divided heads, must also be firmly fixed to the support of the instrument, on opposite sides, and their wires brought to bisect the first dot, and the one which should be 180° distant. Now, the microscopes remaining fixed, turn the circle half round, or until the first microscope coincides with the opposite dot; and, if the other microscope be exactly at the other dot, it is obvious that these dots are 180° apart, or in the true diameter of the circle; and if they disagree, it is obvious that half the quantity by which they disagree, as measured by the divisions of the micrometer head, is the error of the opposite division; for the quantity measured is that by which the greater portion of the circle exceeds the less. It is convenient to note these errors + or —, as the dots are found too forward or too backward, according to the numbering of the degrees; and for the purpose

of distinguishing the + and — errors, the heads, as mentioned before, are numbered backwards and forwards to fifty. One of the microscopes remaining as before, remove the other to a position at right angles; and, considering for the present both the former dots to be true, examine the others by them; *i. e.* as before, try by the micrometer how many divisions of the head the greater half of the semi-circle exceeds the less, and note half the quantity + or —, as before, and do the same for the other semi-circle. One of the micrometers must now be set at an angle of 45° with the other, and the half differences of the two parts of each of the four quadrants registered with their respective signs. When the circle is a vertical one, as in the present instance, it is much the best to proceed so far in the examination with it in that position, for fear of any general bending or spring of the figure; but, for the examination of smaller arcs than 45°, it will be perfectly safe, and more convenient, to have it horizontal; because the dividing apparatus will then carry the micrometers, several perforations being made in the plate B for the limb to be seen through at proper intervals. The micrometers must now be placed at a distance of 22° 30′, and the half differences of the parts of all the arcs of 45° measured and noted as before; thus descending by bisections to 11° 15′, 5° 37′ 30″, and 2° 48′ 45″. Half this last quantity is too small to allow the micrometers to be brought near enough; but it will have the desired effect, if they are placed at that quantity and its half, *i. e.* 4° 13′ 7″.5; in which case the examination, instead of being made at the next, will take place at the next division but one, to that which is the subject of trial. During the whole of the time that the examination is made, all the dots, except the

one under examination, are for the present supposed to be in their true places; and the only thing in this most important part of the business, from first to last, is to ascertain with the utmost care, in divisions of the micrometer head, how much one of the parts of the interval under examination exceeds the other, and carefully to tabulate the half of their difference.

I will suppose that every one, who attempts to divide a large astronomical instrument, will have it engraved first. Dividing is a most delicate operation, and every coarser one should precede it. Besides, its being numbered is particularly useful to distinguish one dot from another; thus, in the two annexed tables of errors, the side columns give significant names to every dot, in terms of its value to the nearest tenth of a degree, and the mistaking of one for another is rendered nearly impossible.

The foregoing examination furnishes materials for the construction of the table of half differences, or apparent errors.* The first line of this table consists of two varieties; *i. e.* the micrometers were at 180° distance for obtaining the numbers which fill the columns of the first and third quadrant; and at 90°, for those of the second and fourth quadrant. The third variety makes one line, and was obtained with a distance of 45°: the fourth consists of two lines, with a distance of 22° 30′: the fifth of four lines, with a distance of 11° 15′: the sixth of eight lines, with a distance of 5° 37′ 30″: the seventh of sixteen lines, with a distance of 2° 48′ 45″: and the eighth and

* If the table of real errors be computed as the work of examination proceeds, there will be no occasion for this table at all; but, I think it best not to let one part interfere with another, and therefore I examine the whole before I begin to compute.

last variety, being the remainder of the table, consists of thirty two lines, and was obtained with a distance of 4° 13′ 7″.5.

The table of apparent errors, or half differences, just explained, furnishes data for computing the table of real errors. The rule is this; let a be the real error of the preceding dot, and b that of the following one, and c the apparent error, taken from the table of half differences, of the dot under investigation; then is $\frac{a+b}{2} + c =$ its real error. But, as this simple expression may not be so generally understood by workmen as I wish, it may be necessary to say the same thing less concisely. If the real errors of the preceding and following dots are both +, or both —, take half their sum and prefix thereto the common sign; but, if one of them is +, and the other —, take half their difference, prefixing the sign of the greater quantity: again, if the apparent error of the dot under investigation has the same sign of the quantity found above, give to their sum the common sign, for the real error; but if their signs are contrary, give to their difference the sign of the greater for the real error. I add a few examples.

Example 1.

For the first point of the second quadrant.	
Real error of the first point of the first quadrant -	0,0
Real error of the first point of the third quadrant	— 6,9
Half sum or half difference - -	— 3,4
Apparent error of the dot under trial -	+ 12,2
Real error - - - -	+ 8,8

Example 2.

For the point 45° of the second quadrant.

Real error of the first point of the quadrant -	+ 8,8
Real error of the last point of the quadrant -	— 6,9
Half difference - - - -	+ 0,9
Apparent error of the dot under trial -	— 8,9
Real error - - - - -	— 8,0

Example 3.

Point 88°,6, or last point, of the third quadrant.

Real error of the point 84°,4 of the third quadrant	— 21,0
Real error of the point 2°,8 of the fourth quadrant	— 2,9
Half sum - - - -	— 11,9
Apparent error of the dot under trial -	— 4,0
Real error - - - - -	— 15,9

Example 4.

Point 88°,6, or last, of the fourth quadrant.

Real error of the point 84°,4 of the fourth quadrant	— 21,6
Real error of the point 2°,8 of the first quadrant -	— 10,2
Half sum - - - - -	— 15,9
Apparent error of the dot under trial -	+ 9,5
Real error - - - - -	— 6,4

It is convenient, in the formation of the table of real errors, that they should be inserted in the order of the numbering of the degrees on their respective quadrants; although their computation necessarily took place in the order in which the examination was carried on, or according to the arrangement in the table of apparent errors. The first dot of the first

quadrant having been assumed to be in its true place, the first of the third quadrant will err by just half the difference found by the examination; therefore these errors are alike in both tables. The real error of the first dot of the second quadrant comes out in the first example; that of the fourth was found in like manner, and completes the first line. It is convenient to put the error of the division 90° of each quadrant at the bottom of each column, although it is the same as the point 0° on the following quadrant. The line of 45° is next filled up; the second example shews this; but there is no occasion to dwell longer upon this explanation; for every one, who is at all fit for such pursuits, will think what has already been said fully sufficient for his purpose. However, I will just mention that there can be no danger, in the formation of this table, of taking from a wrong line the real errors which are to be the criterion for finding that of the one under trial; because they are in the line next to it; the others, which intervene in the full table, not being yet inserted. The last course of all is, however, an exception; for, as the examining microscopes could not be brought near enough to bisect the angle 2° 48′ 43″, recourse was had to that quantity and its half; on which account the examination is prosecuted by using errors at two lines distance, as is shewn in the two last examples.

When the table of real errors is constructed, the other table, although it is of no further use, should not be thrown away; for, if any material mistake has been committed, it will be discovered as the operation of dividing is carried on; and, in that case, the table of apparent errors must be had recourse to; indeed, not a figure should be destroyed until the work is done.

Respecting the angular value of the numbers in these tables, it may be worth mentioning, that it is not of the least importance; 100 of them being comprised in one revolution of the micrometer screw; and, in the instance before me, 5,6 of them made no more than a second. It is not pretended that one of these parts was seen beyond a doubt, being scarcely $\frac{1}{20000}$ of an inch, much less the tenths, as exhibited in the tables; but, as they were visible upon the micrometer heads, it was judged best to take them into the account.

Having now completed the two first sections of my method of dividing; namely, the first, which consists of making 256 small round dots; and the second, in finding the errors of those dots, and forming them into a table; I come now to the third and last part, which consists in using the erroneous dots in comparison with the tabulated errors, so as ultimately to make from them the true divisions.

It will here be necessary to complete the description of the remaining part of the apparatus. And first, a little instrument which I denominate a subdividing sector presents itself to notice. From all that has hitherto been said, it must have been supposed that the roller itself will point out, upon the limb of the instrument to be divided, spaces corresponding to others previously divided upon itself, as was done in setting off the 256 points: but, to obviate the difficulty of dividing the roller with sufficient exactness, recourse was had to this sector; which also serves the equally important purpose of reducing the bisectional points to the usual division of the circle. This sector is represented in full dimensions by Fig. 5: it is formed of thin brass, and centred upon the axis at A, in contact with the upper surface of the roller: it is capable of

being moved round by hand; but, by its friction upon the axis and its pressure upon the roller, it is sufficiently prevented from being disturbed by accident. An internal frame B B, to which the arc C C is attached, moves freely in the outer one, and by a spring D is pushed outwards, while the screw E, whose point touches the frame B, confines the arc to its proper radius. The arc of this sector is of about four times greater radius than the roller, and upon it are divided the spaces which must be transferred to the instrument, as represented on a magnified scale by Fig. 4. Now, the angle of one of the spaces of the circle will be measured by sixteen times its angular value upon the sectorial arc, or 22° 30′; but this does not represent any number of equal parts upon the instrument, whose subdivisions are to be 5′ each; for $\frac{1°\,24'\,22''.5}{5}$ is exactly $16\frac{7}{8}$, therefore so many divisions are exactly equal to a mean space between the dots whose errors have been tabulated. Let, therefore, the arc of the sector be divided into 16 spaces of 1° 20′ each, and let a similar space at each end be subdivided into eight parts of 10′ each, as in Fig. 4; we shall then have a scale which furnishes the means for making the true divisions, and an immediate examination at every bisectional point.

I have always divided the sector from the engine, because that is the readiest method, and inferior to none in point of accuracy, where the radius is very short; but, as it is more liable than any other to centrical error, the adjustment of the arc by the screw E becomes necessary: by that adjustment, also, any undue run in the action of the roller may be reduced to an insensible quantity.*

* See note page 130.

When the utmost degree of accuracy is required, I give the preference to dividing by lines, because they are made with a less forcible effort than dots are; and also because, if any small defect in the contexture of the metal causes the cutter to deviate, it will, after passing the defective part, proceed again in its proper course, and a partial crookedness in the line will be the only consequence; whereas a dot, under similar circumstances, would be altogether displaced. But, on the other hand, where accuracy has been out of the question, and only neatness required, I have used dots; and I have done so, because I know that when a dot and the wire which is to bisect it are in due proportion to each other, (the wire covering about two thirds of the dot) the nicest comparison possible may be obtained. It may be further observed, that division by lines is complete in itself; whereas that by dots requires lines to distinguish their value.

On the upper side of Fig. 1 is represented the apparatus for cutting the divisions. It consists of three pieces JKL, jointed together so as to give to the cutter an easy motion for drawing lines directly radiating from the centre, but inflexible with respect to lateral pressure; *d d* are its handles. The cutting point is hidden below the microscope H; it is of a conical form, and were it used as a dotting point, it would make a puncture of an elliptical shape, whose longer diameter would point towards the centre. This beautiful contrivance, now well known, we owe to the ingenuity of the late Mr. HINDLEY of York; it was borrowed by Mr. RAMSDEN,* and applied with the best effect to his dividing engine.

* This I learned from that most accurate artist Mr. JOHN STANCLIFFE, who was himself apprentice to HINDLEY.

It might have been mentioned sooner, that in the instance which I have selected as an example of my dividing, the operation took place when the season of the year, and the smoke of London, had reduced the day to scarcely six hours of effective light; and rather than confine my labours within such narrow limits, I determined to shut out the day-light altogether. Fig. 7 shews the construction of the lanterns which I used. A very small wick gave sufficient light, when kept from diverging by a convex lens; while the inclining nossel was directed down exactly upon the part looked at, and the light, having also passed through a thin slice of ivory, was divested of all glare. I enter into this description, because, I think, I never saw my work better, nor entirely to so much advantage as in this instance; owing, perhaps, to the surrounding darkness allowing the pupil of the eye to keep itself more expanded, than when indirect rays are suffered to enter it. The heat from a pair of these lanterns was very inconsiderable, and chiefly conducted along with the smoke up the reclining chimney.

Previous to cutting the divisions, the parts now described must be adjusted. The cutting apparatus must be placed with the dividing point exactly at the place where the first line is intended to be drawn, and clamped, so that the adjusting screw may be able to run it through a whole interval. The microscope H must be firmly fixed by its two pillars *b b* to the main frame, with its micrometer head at *zero*; and with its only wire in the line of the radius, bisecting the first of the 256 dots. And it should be observed, that the cutting frame and this must not vary respecting each other, during the time that the divisions are cut; for any motion that took place in either

would go undiminished to the account of error. The microscope I is also fastened to the main frame; but it is only required to keep its position unvaried, while the divisions of the sector pass once under its notice; for it must have its wires adjusted afresh to these divisions at every distinct course. The microscope I has two wires, crossing each other at an angle of about 40°; and these are to be placed so as to make equal angles with the divisions of the sector, which are not dots, but lines. The sectorial arc must also be adjusted to its proper radius by the screw E, Fig. 5; *i. e.* while the main frame has been carried along the circle through a mean interval shewn by H, the sector must have moved through exactly 16$\frac{1}{2}$ of its divisions, as indicated by I.*

Things being in this position; after having given the parts time to settle, and having also sufficiently proved the permanence of the micrometer H and the cutting frame with respect to each other, the first division may be made; then, by means of the screw for slow motion, carry the apparatus forward, until the next line upon the sector comes to the cross wires of I; you then cut another division, and thus proceed until the 16th division is cut, = 1° 20′: Now the apparatus wants to be carried

* For the sake of simplicity, the account of the process is carried on as if the roller measured the mean interval without error: But it was said (Page 107) that the roller, in a continued motion quite round the circle, would in some part of its course err by 30″ or more; therefore, when that is the case, an extreme run of the roller cannot agree with a mean interval of the circle nearer than $\frac{30''}{128} = 0'',23$; and most probably this kind of error will on some intervals amount to double that quantity. It therefore becomes matter of prudent precaution to examine every interval previous to making the divisions; and, where necessary, to adjust the sector, so that its arc may exactly measure the corresponding interval as corrected by the tabulated errors.

further, to the amount of $\frac{3}{7}$ of a division, before an interval is complete; but at this last point no division is to be made; we are here only to compare the division on the sector with the corresponding dot upon the instrument: This interval, however, upon the circle will not be exactly measured by the corresponding line of the sector, which has been adjusted to the mean interval, for the situation of the dot 1°,4 is too far back, as appears by the table of real errors, by — 4,8 divisions of the micrometer head. The range of the screw for slow motion must now be restored, the cross wires of H set back to — 4,8 divisions, and the sector moved back by hand, but not to the division 0 where it began before; for, as it left off in the first interval at $\frac{3}{7}$ of a division, it has to go forwards $\frac{4}{7}$ more before it will arrive at the spot where the 17th division of the instrument 1° 25′ is to be made, so that in this second course it must begin at $\frac{4}{7}$ short of 0: Go through this interval as before, making a division upon the circle at every one of the 16 great divisions of the sector; and H should now reach the third dot, allowing for a tabular error of — 10,2 when the division $\frac{4}{7}$ths of the sector reaches the cross wires of I. It would be tedious to lead the reader through all the variety of the sector, which consists of eight courses; and it may be sufficient to observe, that at the commencement of every course, it must be put back to the same fraction of a division which terminated its former one; and that the wire of the micrometer H must always be set to the tabular error belonging to every dot, when we end one interval and begin another. The eight courses of the sector will have carried us through $\frac{1}{32}$ part of the circle, 11° 15′, and during this time, the roller will have proceeded through half a revolution; for

its close contact with the limb of the circle does not allow it to return with the sector when the latter is set back at every course. Having in this manner proceeded, from one interval to another, through the whole circle, the micrometer at last will be found with its wire, at *zero*, on the dot from which it set out; and the sector, with its 16th division, coinciding with the wires of its microscope.

Having now given a faithful detail of every part of the process of dividing this circle, I wish to remind the reader that, by verification and correction at every interval, any erroneous action of the roller is prevented from extending its influence to any distant interval. It will be further observed, that the subdividing sector magnifies the work; that by means of its adjustable arc, it makes the run of the roller measure its corresponding intervals upon the circle; and, without foreign aid, furnishes the means of reducing the bisectional intervals to the usual division of the circle. Furthermore, the motion of the wire of the micrometer H, according to the divisions of its head and corresponding table of errors, furnishes the means of prosecuting the work with nearly the same certainty of success, as could have happened, had the 256 points been (which in practice is quite impossible) in their true places.

Now, the whole of my method of dividing being performed by taking short measures with instruments which cannot themselves err in any sensible degree, and, inasmuch as those measures are taken, not by the hand, but by vision, and the whole performed by only looking at the work, the eye must be charged with all the errors that are committed until we come to cut the divisions; and, as in this last operation the

hand has no more to do than to guide an apparatus so perfect in itself, that it cannot be easily made to deviate from its proper course, I would wish to distinguish it from the other methods by denominating it, DIVIDING BY THE EYE.*

The number of persons at all capable of dividing originally have hitherto been very few; the practice of it being so limited, that, in less than twice seven years, a man could hardly hope to become a workman in this most difficult art. How far I shall be considered as having surmounted these difficulties, I know not; but if, by the method here revealed,

* I must here remark, that SMEATON has represented the greatest degree of accuracy that can be derived from vision, in judging of the coincidence of two lines at $\frac{1}{1000}$ part of an inch. From this it may fairly be inferred, that he had not cultivated the power of the sight, as he had done that of the touch; the latter of which, with that ability which appeared in all his works, he rendered sensible to the $\frac{1}{10000}$ part of an inch. Were materials infinitely hard, no bounds could be set to the precision of contact; but taking things as they are, the different degrees of hardness in matter, may be considered as a kind of magnifying power to the touch, which may not unaptly be compared with the assistance which the eye receives from glasses. It is now quite common to divide the seaman's sextant to 10″, and a good eye will estimate the half of it; which, on an eight inch radius is scarcely $\frac{1}{10000}$ of an inch. This quantity, small as it is, is rendered visible by a glass of one inch focal length; and such is the certainty with which these quantities are seen, that a seaman will sometimes complain that two pair of these lines will coincide at the same time; and that may happen, and yet no division of his instrument err, by more than $\frac{1}{10000}$ part of an inch. All this is applicable to judging of the coincidence of *lines* with each other, and furnishes not the most favourable display of the accuracy of vision. But with the microscopes here described, where the wire bisects the image of a dot, or a cross wire is made to intersect the image of a line, by an eye practised in such matters, a coincidence may undoubtedly be ascertained to $\frac{1}{10000}$ part of an inch. I am of opinion that as small a quantity may be rendered visible to the eye, as can by contact be made sensible to the touch; but whether Mr. SMEATON's $\frac{1}{10000}$ and my $\frac{1}{10000}$ be not the same thing, I will not determine; the difference between them, however, is what he would no more have pretended to feel, than I dare pretend to see.

I have not rendered original dividing almost equally easy with what copying was before, I have spent much labour, time, and thought in vain. I have no doubt indeed, that any careful workman who can divide in common, and has the ability to construct an astronomical instrument, will, by following the steps here marked out, be able to divide it, the first time he tries, better than the most experienced workman, by any former method.

If, instead of subdividing with the roller, the same thing be performed with the screw, it will not give to dividing by the eye any very distinctive character: I have practised this on arcs of circles with success, the edge being slightly racked, the screw carrying forward an index with the requisite apparatus, and having a divided micrometer head; the latter answers to the subdividing sector, and, being used with a corresponding table of errors, forms the means of correcting the primitive points; but the roller furnishes a more delicate action, and is by far more satisfactory and expeditious.

It is known to many that the six feet circle, which I am now at work upon for our Royal Observatory, is to be divided upon a broad edge, or upon a surface at right angles to the usual plane of division: The only alterations, which will on that account be required, are, that the roller must act upon that plane which is usually divided upon; which roller, being elevated or depressed, may be adjusted to the commensurate radius without being made conical, as was necessary in the other case. The apparatus, similar to the other, must here be fixed immoveable to the frame which supports the circle; its position must be at the vertex, where also I must have my station; and the instrument itself must be turned around its axis, in its

proper vertical position, as the work proceeds. The above may suffice, for the present, to gratify those who feel themselves interested upon a subject which will be better understood, if I should hereafter have the honour of laying before the Royal Society a particular description of the instrument here alluded to; a task which I mean to undertake, when, after being fixed in the place designed for it, which I hope will be effected at no very distant period, it shall be found completely to answer the purposes intended.

Should it be required to divide a circle according to the centesimal division of the quadrant, as now recommended and used in France, we shall have no difficulty. The 100° of the quadrant may be conveniently subdivided into 10 each, making 4000 divisions in the whole round. The 256 bisectional intervals, the two tables of errors, and the manner of proceeding and acting upon them will be exactly the same as before, until we come to cut the divisions; and for this purpose we must have another line divided upon the sector. For $\frac{1}{4000}$ part of the circle being equal to $5',4$ of the usual angular measure $\frac{1°\,24'\,22'',5}{5',4} = 15\frac{1}{5}$ divisions; and just so many will be equivalent to one of the intervals of the circle. The value of one of the great divisions of the sector will be 1° 26′ 24″, and that of the $\frac{1}{4}$ parts, which are to be annexed to the right and left as before, will be 10′ 48″, therefore divisible by the engine. Should any astronomer choose to have both graduations upon his instrument, the additional cost would be a mere trifle, provided both were done at the same time.

It must already have been anticipated, that dividing by the eye is equally applicable to straight lines as it is to circles.

An apparatus for this purpose should consist of a bar of brass, three quarters of an inch thick, and not less than three inches broad; six feet may do very well for the length; it may be laid upon a deal plank strengthened by another plank screwed edgewise on its lower surface. The bar should be planed, on both its edges and on its surface, with the greatest exactness; and it will be better, if it has a narrow slip of silver, inlaid through its whole length, for receiving the dots. An apparatus nearly similar to the other should slide along its surface, carrying a roller, whose circumference is 12,8 inches, and turned a little conical for the sake of adjustment. The roller may be divided into 32 parts, each of which when transferred to the bar will give intervals of 0,4 of an inch each: The angle of the subdividing sector should of course be 11° 15′, and subdivided into four parts, which will divide the inch into tenths: The surface may also receive other lines, with subdivisions suited to the different purposes for which it may be wanted. The revolutions of the roller and its $\frac{1}{32}$ parts must be dotted upon the bar; taking care, by sizing the roller, to come as near the true standard measure as possible: When this is done, compare the extent of the greater bisectional number that is contained in the length; *i. e.* 128 intervals or 51,2 inches, with the standard measure; noting the difference as indicated by the micrometer heads: The examination and construction of the table of errors may then be conducted just as was done for the circle.

Being now ready for the performance of its work, the scale to be divided must be laid alongside of the bar, and the true divisions must be cut upon it by an appeal, as before, to the erroneous dots on the bar, corrected by a corresponding table

of errors. The apparatus, remaining entire in the possession of the workman, with its primitive dots, the table of errors, &c. is ready for dividing another standard, which will be precisely similar to others that have been, or may be, divided from it. It may be considered, indeed, as a kind of engine; and, as it is not vitiated by the coarse operation of racking with a screw, but performed by only looking at the work, the method will command about three times the accuracy that can be derived from the usual straight-line dividing engine. Should it be asked, if an engine thus appointed would succeed for dividing circles? I answer, Yes; but I would not recommend it; because, beyond a certain extent of radius, it is not necessary; for the errors, which would be introduced into the work by the violence of racking a large wheel, are sufficiently reduced by the comparative shortness of the radius of such instruments as we divide by that method: And, what is still more to the purpose, the dividing engine is four times more expeditious, and bears rough usage better. I cannot quit the subject of dividing straight lines without observing, that I never had my apparatus complete. The standard which I made for Sir GEORGE SHUCKBURG EVELYN in 1796 was done by a mere make-shift contrivance, upon the principle of dividing by the eye; how I succeeded may be seen in Sir GEORGE's papers on Weights and Measures (Phil. Trans. for 1798). I made a second, some years after, for Professor PICTET of Geneva, which became the subject of comparison with the new measure of France, before the National Institute; and their report, drawn up by Mr. PICTET, has been ably re-stated and corrected by Dr. YOUNG, as published in the Journals of the Royal Institution. I made a third for the Magistrates of

Aberdeen. I notice the two latter, principally to give myself an opportunity of saying that, if those three scales were to be compared together, notwithstanding they were divided at distant periods of time, and at different seasons of the year, they would be found to agree with each other, as nearly as the different parts of the same scale agree.

I hope I may here be allowed to allude to an inadvertency which has been committed in the paper mentioned above; and which Sir GEORGE intended to have corrected, had he lived to conclude his useful endeavours to harmonize the discordant weights and measures of this country. The instruments which he has brought into comparison are, his own five feet standard measure and equatorial; General ROY's forty-two inch scale; the standard of Mr. AUBERT; and that of the Royal Society. The inadvertency is this: In his equatorial, and the standard of the Royal Society, he has charged the error of the most erroneous extent, when compared with the mean extent, alike to both divisions; *i. e.* he has supposed one of the divisions, which bound the erroneous extent, to be too much to the right, and the other too much to the left, and that by equal quantities: This is certainly a good natured way of stating the errors of work; and perhaps not unjustly so, where the worst part has been selected; but, in the other three instances, namely, in General ROY's, Mr. AUBERT's, and his own standard, he has charged the whole error of the most erroneous extent to one of the bounding lines.

I was well confirmed in my high opinion of the general accuracy of BIRD's dividing, when, last winter,* I measured the chords of many arcs of the Greenwich quadrant: That instrument has indeed suffered both from a change in its figure,

* This paper was written in June 1808.

and from the wearing of its centre; but the graduation, considering the time when it was done, I found to be very good. Sir George in his Paper upon the Equatorial (Phil. Trans. for 1798), after some compliments paid to the divider of his instrument, says, "the late Mr. John Bird seems to have admitted a probable discrepancy in the divisions of his eight "feet quadrant amounting to 3″;" and he refers to Bird on the construction of the Greenwich quadrant. This quantity being three times as great as any errors that I met with, I was lately induced to inquire how the matter stood. Bird, in the paper referred to, says, "in dividing this instrument I "never met with an inequality that exceeded one second. I "will suppose that in the 90 arch this error lay towards the "left hand, and in the 96 arch that it lay towards the right, "it will cause a difference between the two arches of two "seconds; and, if an error of one second be allowed to the "observer in reading off his observation, the whole amount "is no more than three seconds, which is agreeable to what "I have heard, &c." Sir George's examination of his own Equatorial furnishes me with the means of a direct comparison: In his account of the declination circle, we find an error + 2″,35, and another — 1″,5; to these add an error of half a second in each, for reading off, which Sir George also admits, we shall then have a discrepancy of 4″,85; but, as the errors of reading off are not errors of division, let them be discharged from both, and the errors will then stand, for the quadrant 2″, and for the circle 3″,85. As the radius of the former, however, is four times greater than that of the latter, it will appear, by this mode of trial, that the Equatorial is rather more than twice as accurately divided as the quadrant.

In doing justice to BIRD in this instance, I have only done as I would be done by; for, should any future writer set me back a century on the chronological scale of progressive improvement, I hope some one will be found to restore me to my proper niche. I now subjoin a re-statement of the greatest error of each of the instruments that are brought into comparison by Sir GEORGE, after having reduced them all by one rule; viz. allowing each of the two points which bound the most erroneous extent to divide the apparent error equally between them. They are expressed in parts of an inch, and follow each other in the order of their accuracy.

Sir GEORGE SHUCKBURG's 5 feet standard	,000165
General ROY's scale of 40 inches - -	,000240
Sir GEORGE's Equatorial, 2 feet radius -	,000278
The Greenwich quadrant, 8 feet radius -	,000465
Mr. AUBERT's standard, 5 feet long -	,000700
*The Royal Society's standard 90 inches long	,000795

For the justness of the above statement I consider my name as pledged; requesting the permission to say, that if on the result of each respective examination, as here presented, there could have been more than one opinion, it would not have appeared here. I am further prompted to add, that the above comparative view presents one circumstance to our notice, which cannot do less than gratify every individual who is at all conversant in these matters; I mean, the high rank which General ROY's scale takes in the list; that scale having been made the agent in measuring the base line of our national trigonometrical survey.

* This is the same which Mr. BIRD used in dividing his eight feet mural quadrant, and was presented to the Royal Society by BIRD's executors.

To return, finally, to the dividing of circles; I must state, as matter of precaution, that great care should be taken during the turning of the outer edge, to have the circle of the same temperature; for one part may be expanded by heat, or contracted by cold, so much more than another, as to cause the numbers in the tables of errors to be inconveniently large. A night is not more than sufficient for allowing the whole to take the same temperature, after having been handled by the workmen; and the finishing touch should be given within a short space of time. But, if the effects of temperature are to be regarded in turning a circle, it is of tenfold more importance to attend to this circumstance, while the examination of the larger arcs of the instrument is carried on; for it is absolutely necessary that, during this time, the whole circle should be of the same heat exactly. Few workmen are sufficiently aware of this: They generally suppose the expansion of metals to be a trifle which need not be regarded in practice; and wonder how the parts of a circle can be differently heated without taking pains to make it so. One degree of FAHRENHEIT's thermometer indicates so small a portion of heat that, in such places as workmen are usually obliged to do their business in, it is not very easy to have three thermometers attached to different parts of a large instrument, shewing an equality of temperature within that quantity: Yet so necessary is correctness in this respect, that if a circle has the vertex one degree warmer than its opposite, and if this difference of temperature be regularly distributed from top to bottom, the upper semi-circle will actually exceed the lower by 2″: And, if such should happen to be the case while the examination of the first dot of the third quadrant is made,

the regularity of the whole operation would thereby be destroyed.

It may not be improper to remark, that dividing by the eye does not require a more expensive apparatus than the operation of dividing by hand; and, indeed, less so when the scale of inches is deemed necessary. The method by adjustment is still more expensive, requiring whatever tools BIRD's method requires, and, in addition to these, a frame and microscopes, somewhat similar to those for dividing by the eye.

It is somewhat more difficult to give a comparative estimate of the time which the different methods of dividing require. I know that thirteen days of eight hours each, are well employed in dividing such a circle by my method; about fifty-two days would be consumed in doing the same thing by BIRD's method; and I think I cannot err much when I state the method by adjustment, supposing every dot to be tried, and that two-thirds of them want adjusting, to require about one hundred and fifty of such days.

The economy of time (setting aside the decided means of accuracy) which the above estimate of its application offers to view, will, I think, be considered of no little moment. By the rising artist who may aspire at excellence, it will at least, and I should hope, with gratitude, be felt in the abbreviation of his labours. To me, indeed, the means of effecting this became indispensible; and it has not been without a sufficient sense of its necessity, that I have been urged to the progressive improvement and completion of these means, as now described. It is but little that a man can perform with his own hands alone; nor is it on all occasions, even in frames of firmer texture than my own, that he can decisively command their

adequate, unerring, use. And I must confess that I never could reconcile it to what I hold as due to myself, as well as to a solicitous regard for the most accurate cultivation of the science of astronomy, to commit to others an operation requiring such various and delicate attentions, as the division of my instruments.

That my attentions on this head have not failed to procure for me the notice and patronage of men whose approbation makes, with me, no inconsiderable part of my reward, I have to reflect on with gratitude and pleasure: And as I look with confidence to the continuance of that patronage so long as the powers of execution shall give me the inclination to solicit it, I cannot entertain a motive which might go to extinguish the more liberal wish of pointing out to future ingenuity a shorter road to eminence; sufficiently gratified by the idea of having in the present communication, contributed to facilitate the operations, and to aid the progress of art (as far as the limited powers of vision will admit) towards the point of perfection.

Table of apparent Errors.

Name of the Dot.	First Quadrant.	Second Quadrant.	Third Quadrant.	Fourth Quadrant.	First Quadrant.	Second Quadrant.	Third Quadrant.	Fourth Quadrant.	Name of the Dot.
°									°
0.0	0	+ 12.2	− 6.9	+ 17.9	+ 4.6	+ 17.1	− 4.4	+ 17.3	1.4
45.0	− 21.3	− 8.9	16.7	− 29.6	− 5.2	− 9.7	8.9	− 6.4	4.2
22.5	1.6	2.2	1.0	2.7	0.0	3.8	1.0	4.7	7.0
67.5	+ 1.0	+ 15.6	0.0	+ 13.7	+ 1.0	+ 3.5	5.1	5.5	9.8
11.2	− 16.6	− 20.2	22.6	− 30.3	− 5.5	− 1.6	0.0	+ 1.2	12.7
33.7	4.0	4.2	23.2	23.1	7.6	7.6	4.2	− 2.3	15.5
56.2	16.9	22.2	17.0	22.7	9.4	3.9	0.0	5.3	18.3
78.7	30.8	16.6	31.3	30.3	+ 2.1	+ 12.1	+ 4.2	+ 4.3	21.1
5.6	2.7	8.6	4.2	10.1	22.3	0.9	6.2	14.4	23.9
16.9	11.5	11.3	11.2	16.1	− 5.7	− 6.2	1.1	− 11.2	26.7
28.1	9.0	7.4	5.8	14.3	+ 1.5	3.5	− 6.3	4.2	29.5
39.4	9.3	8.2	5.8	13.1	0.0	7.0	7.7	+ 1.4	32.3
50.6	4.2	6.8	8.2	4.4	1.5	+ 9.0	+ 3.0	4.3	35.2
61.9	4.3	8.4	12.5	4.4	− 8.6	− 5.9	− 2.0	− 6.7	38.0
73.1	7.6	10.0	13.6	9.7	3.3	+ 2.7	4.9	1.5	40.8
84.4	18.0	+ 6.0	16.3	7.1	+ 4.0	3.1	3.5	+ 1.0	43.6
2.8	3.4	− 7.5	8.9	2.1	13.5	10.5	+ 10.0	14.9	46.4
8.4	0.0	5.0	4.6	5.7	2.1	0.0	1.7	− 3.5	49.2
14.1	6.6	8.2	5.6	4.8	− 5.0	− 10.7	− 2.9	1.5	52.0
19.7	1.6	2.4	+ 1.0	2.5	4.2	7.9	2.2	7.2	54.8
25.3	3.7	8.2	− 2.9	2.5	4.0	3.0	2.5	1.0	57.7
30.9	+ 2.4	7.1	7.0	0.0	7.3	+ 6.2	6.1	1.5	60.5
36.6	− 5.9	+ 1.0	2.5	1.5	3.2	− 10.1	5.6	12.7	63.6
42.2	+ 3.1	1.9	5.8	+ 2.5	1.4	7.2	3.9	+ 2.2	66.1
47.8	7.1	5.2	+ 2.4	4.8	+ 11.2	+ 14.9	+ 21.2	7.2	68.9
53.4	− 5.6	− 6.0	− 5.0	− 6.1	− 7.1	− 1.0	− 8.9	− 11.7	71.1
59.1	10.7	+ 1.0	3.0	+ 1.4	5.3	1.2	6.6	2.7	74.5
64.7	7.9	− 18.0	10.7	− 9.0	7.2	9.9	+ 1.0	5.9	77.3
70.3	2.7	7.4	1.5	9.0	6.5	1.8	5.3	2.6	80.2
75.9	1.2	5.2	2.2	4.7	+ 4.4	+ 1.4	− 2.2	4.3	83.0
81.6	1.6	+ 1.7	0.0	2.0	− 20.8	− 0.0	11.4	+ 1.0	85.8
87.2	13.7	6.0	3.5	+ 5.6	+ 2.1	+ 11.0	4.0	9.5	88.6

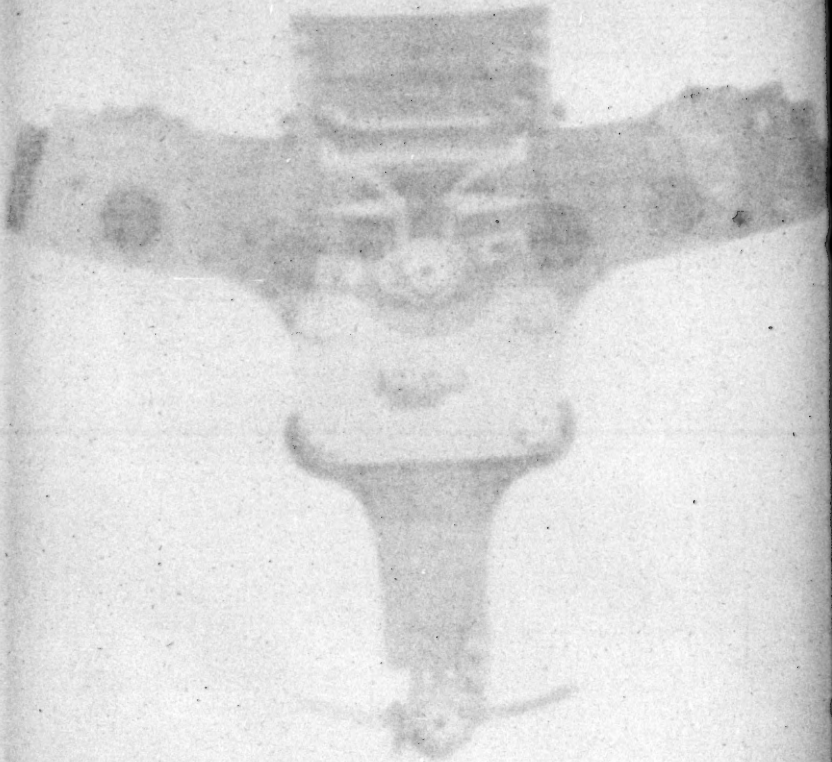
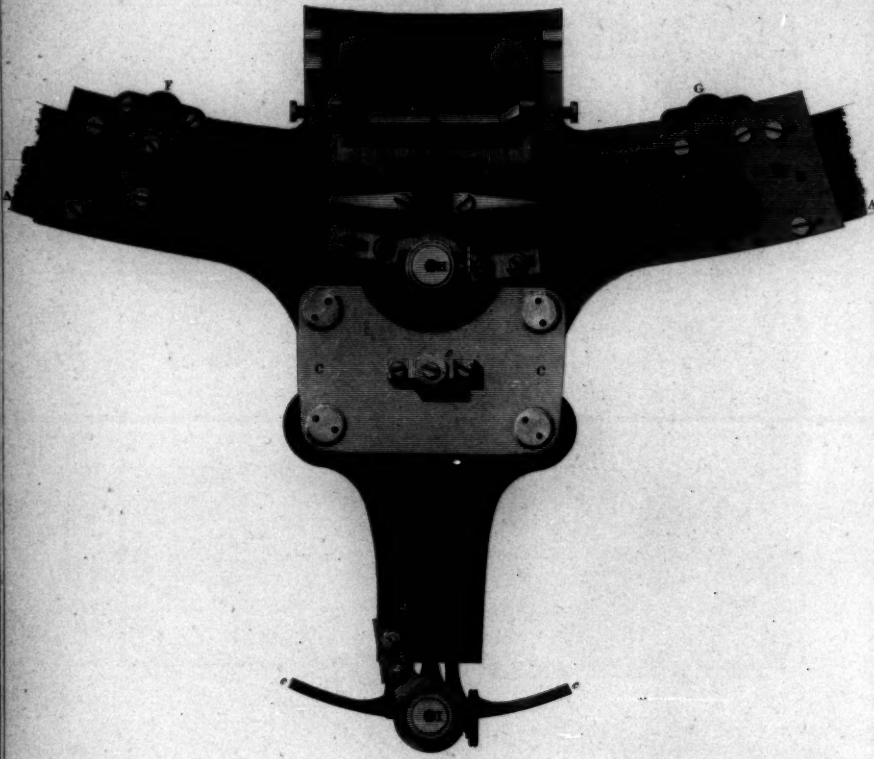


Fig. 1.



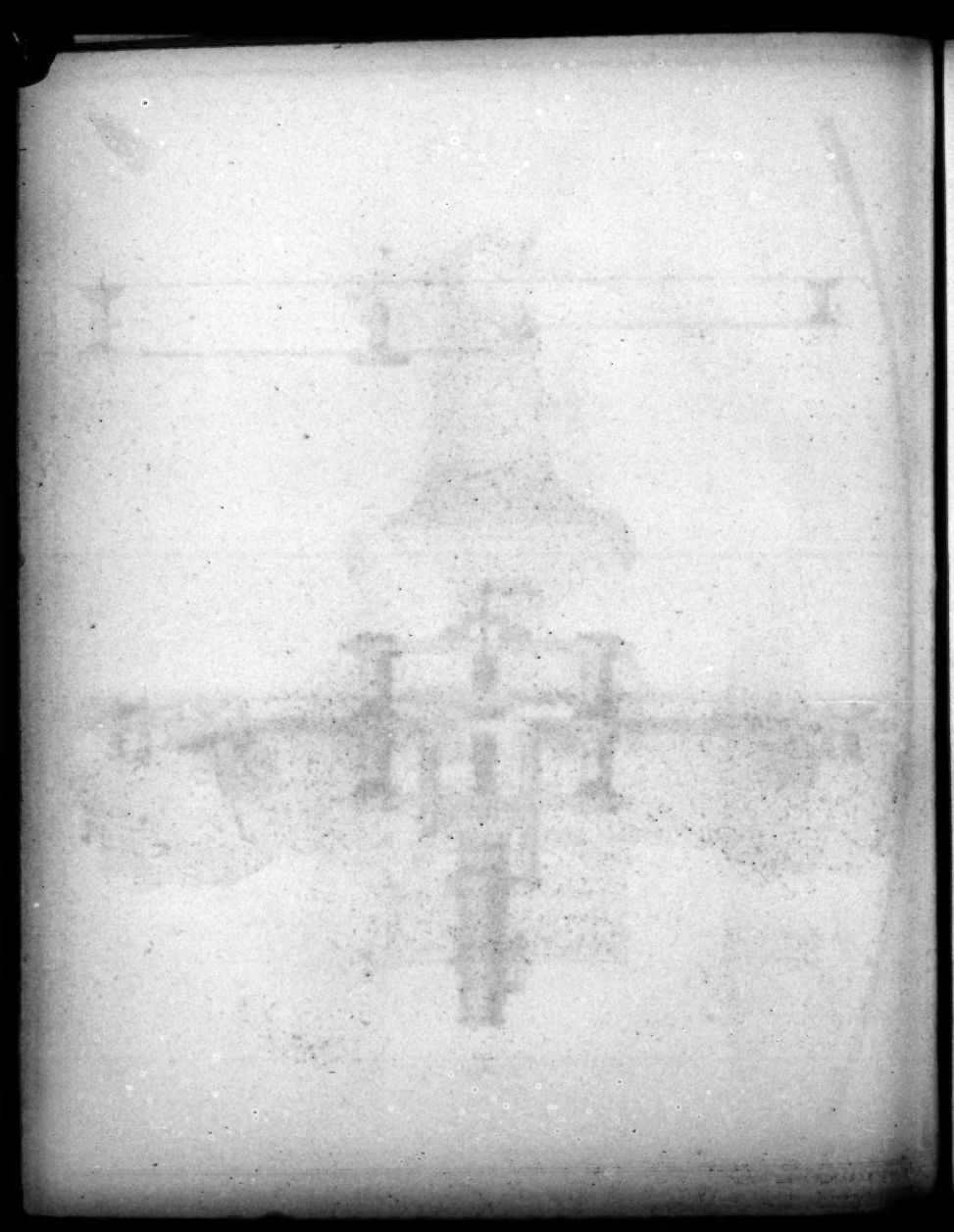


Fig. 2.

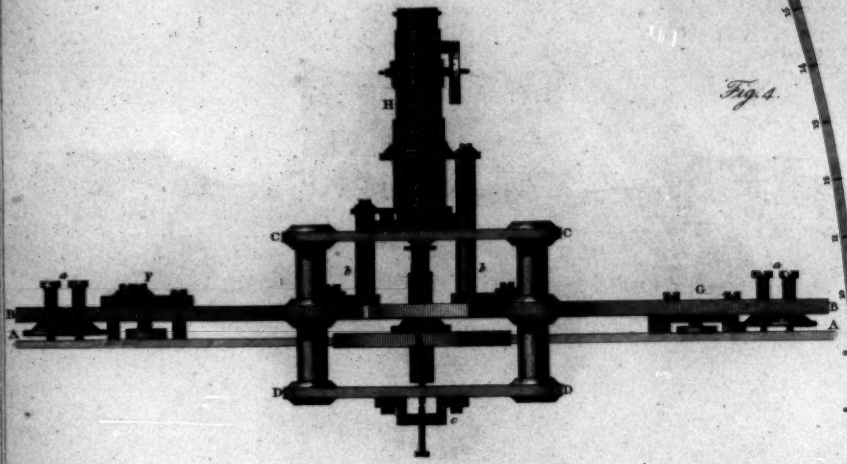


Fig. 4.

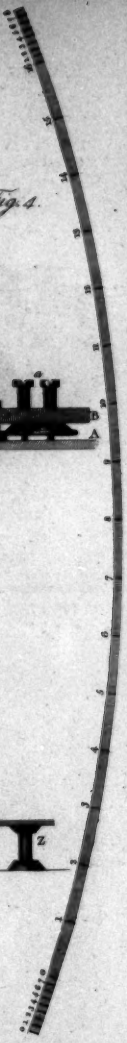


Fig. 3.

Basire sc.

Fig. 5.

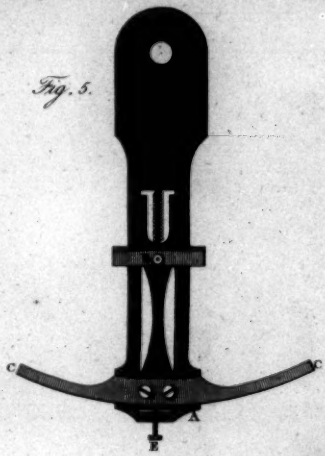


Fig. 6.

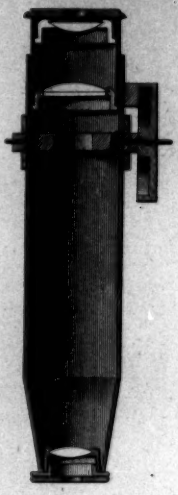


Fig. 7.

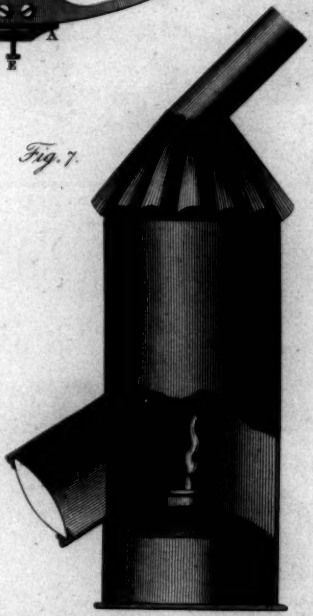


Table of real Errors.

Name of the Dot	First Quadrant.	Second Quadrant.	Third Quadrant.	Fourth Quadrant.	First Quadrant.	Second Quadrant.	Third Quadrant.	Fourth Quadrant.	Name of the Dot
°									°
0,0	0,0	+ 8,8	− 6,9	+ 14,4	−16.9	− 8,0	−13,4	− 22,4	45,0
1,4	− 4,8	− 0,6	16,0	5,9	8,7	5,5	9,7	16,1	40.4
2,8	10,2	9,3	24,0	− 2,9	14,3	9,6	17,4	22,3	47,8
4,2	13,8	15,1	29,3	12,8	22,3	17,9	19,9	33,8	49,2
5,6	13,7	12,5	23,3	16,1	26,0	21,6	20,7	31,9	50,6
7,0	13,8	16,8	28,7	19,4	25,5	26,0	24,6	28,9	52,0
8,4	17,6	19,6	32,0	27,0	32,0	27,8	30,3	38,3	53,4
9,8	22,4	16,1	35,5	30,7	34,0	27,3	29,1	35,2	54,8
11,2	21,6	16,7	31,5	26,5	26,8	22,1	24,0	32,6	56,2
12,7	27,6	21,6	32,2	28,0	29,6	24,5	29,7	29,8	57,7
14,1	31,2	26,8	37,3	34,4	33,7	27,7	27,2	24,6	59,1
15,5	28,5	22,7	30,2	26,8	30,2	15,6	20,3	26,5	60,5
16,9	27,3	20,5	32,4	32,7	19,2	15,3	24,1	19,4	61,9
18,3	29,9	18,2	24,2	25,7	21,5	14,6	18,8	23,7	63,3
19,7	20,2	13,5	20,6	21,2	19,0	21,5	22,4	17,4	64,7
21,1	22,4	5,9	22,1	24,0	18,8	19,9	22,8	17,1	66,1
22,5	10,0	1,8	10,9	8,7	3,0	+ 8,2	+ 0,7	+ 2,5	67,5
23,9	8,8	12,2	16,0	14,9	9,8	− 2,8	− 2,5	−13,0	68,9
25,3	19,8	15,5	20,2	24,0	15,7	10,2	13,7	19,2	70,3
26,7	21,7	10,1	20,0	23,0	21,9	7,0	21,8	25,8	71,7
28,1	22,1	12,8	23,8	30,4	23,0	13,9	25,1	23,0	73,1
29,5	17,1	15,8	28,9	35,0	22,0	14,3	25,3	26,8	74,5
30,9	22,1	18,0	31,4	37,0	26,6	20,1	26,6	30,7	75,9
32,3	24,7	19,3	33,3	37,7	33,3	21,1	22,7	31,1	77,3
33,7	17,4	9,1	25,1	37,6	27,9	16,0	23 8	29,1	78,7
35,2	22,7	8,0	25,1	35,7	35,3	14,5	18,5	28,7	80,2
36,6	27,3	11,9	27,4	41,8	29,3	9,0	22,4	27,3	81,6
38,0	26,5	15,6	26,9	40,6	21,6	6,6	17,5	21,4	83,0
39,4	26,4	16,7	24,8	43,1	27,5	5,4	21,0	21,6	84,4
40,8	25,4	7,2	25,1	33,6	31,0	7,9	15,4	12,6	85,8
42,2	18,5	10,4	24,7	39,2	31,0	0,1	6,8	5,2	87,1
43,6	16,3	10,0	24,6	31,7	26,3	1,7	15,9	6,4	88,6
45,0	16,9	8,0	13,0	22,4	+ 8,6	2,9	+ 14,4	0.0	90,0

V. *A Letter on a Canal in the Medulla Spinalis of some Quadrupeds. In a Letter from Mr.* William Sewell, *to* Everard Home, *Esq. F. R. S.*

Read December 8, 1808.

Sir,

According to your request, I send you an account of the facts I have ascertained, respecting a canal I discovered in the year 1803, in the medulla spinalis of the horse, bullock, sheep, hog, and dog; and should it appear to you deserving of being laid before the Royal Society, I shall feel myself particularly obliged, by having so great an honour conferred upon me.

Upon tracing the sixth ventricle of the brain, which corresponds to the fourth in the human subject, to its apparent termination, the calamus scriptorius, I perceived the appearance of a canal, continuing by a direct course into the centre of the spinal marrow. To ascertain with accuracy whether such structure existed throughout its whole length, I made sections of the spinal marrow at different distances from the brain, and found that each divided portion exhibited an orifice with a diameter sufficient to admit a large sized pin; from which a small quantity of transparent colourless fluid issued, like that contained in the ventricles of the brain. The canal is lined by a membrane resembling the tunica arachnoidea, and is situated above the fissure of the medulla, being separated by a medullary layer: it is most easily distinguished where the large

nerves are given off in the bend of the neck and sacrum, imperceptibly terminating in the cauda equina. Having satisfactorily ascertained its existence through the whole length of the spinal marrow, my next object was to discover whether it was a continued tube from one extremity to the other: this was most decidedly proved, by dividing the spinal marrow through the middle, and pouring mercury into the orifice where the canal was cut across, it passed in a small stream with equal facility towards the brain (into which it entered), or in a contrary direction to where the spinal marrow terminates.

By many similar experiments, I have since proved that a free communication of the limpid fluid, which the canal contains, is kept up between the brain and whole extent of spinal marrow. I have consulted the most celebrated authors on comparative anatomy, but do not find any such structure of those parts described, and as it is not known to you, I may presume that it has not been before taken notice of.

I have the honour to be,

Sir, your obedient faithful servant,

WM. SEWELL.

Veterinary College,
Nov. 26, 1808.

VI. *A numerical Table of elective Attractions; with Remarks on the Sequences of double Decompositions.* By Thomas Young, *M. D. For. Sec. R. S.*

Read February 9, 1809.

ATTEMPTS have been made, by several chemists, to obtain a series of numbers, capable of representing the mutual attractive forces of the component parts of different salts; but these attempts have hitherto been confined within narrow limits, and have indeed been so hastily abandoned, that some very important consequences, which necessarily follow from the general principle of a numerical representation, appear to have been entirely overlooked. It is not impossible, that there may be some cases, in which the presence of a fourth substance, besides the two ingredients of the salt, and the medium in which they are dissolved, may influence the precise force of their mutual attraction, either by affecting the solubility of the salt, or by some other unknown means, so that the number, naturally appropriate to the combination, may no longer correspond to its affections; but there is reason to think that such cases are rare; and when they occur, they may easily be noticed as exceptions to the general rules. It appears therefore, that nearly all the phenomena of the mutual actions of a hundred different salts may be correctly represented by a hundred numbers, while, in the usual manner of relating every case as a different experiment, above two thousand separate articles would be required.

Having been engaged in the collection of a few of the principal facts relating to chemistry and pharmacy, I was induced to attempt the investigation of a series of these numbers; and I have succeeded, not without some difficulty, in obtaining such as appear to agree sufficiently well with all the cases of double decompositions which are fully established, the exceptions not exceeding twenty, out of about twelve hundred cases enumerated by FOURCROY. The same numbers agree in general with the order of simple elective attractions, as usually laid down by chemical authors; but it was of so much less importance to accommodate them to these, that I have not been very solicitous to avoid a few inconsistencies in this respect, especially as many of the bases of the calculation remain uncertain, and as the common tables of simple elective attractions are certainly imperfect, if they are considered as indicating the order of the independent attractive forces of the substances concerned. Although it cannot be expected that these numbers should be accurate measures of the forces which they represent, yet they may be supposed to be tolerable approximations to such measures, at least if any two of them are nearly in the true proportion, it is probable that the rest cannot deviate very far from it: thus, if the attractive force of the phosphoric acid for potash is about eight tenths of that of the sulfuric acid for barita, that of the phosphoric acid for barita must be about nine tenths as great; but they are calculated only to agree with a certain number of phenomena, and will probably require many alterations, as well as additions, when all other similar phenomena shall have been accurately investigated.

There is, however, a method of representing the facts, which

have served as the bases of the determination, independently of any hypothesis, and without being liable to the contingent necessity of any future alteration, in order to make room for the introduction of the affections of other substances; and this method enables us also to compare, upon general principles, a multitude of scattered phenomena, and to reject many which have been mentioned as probable, though doubtful, with the omission of a very few only which have been stated as ascertained. This arrangement simply depends on the supposition, that the attractive force, which tends to unite any two substances, may always be represented by a certain constant quantity.

From this principle it may be inferred, in the first place, that there must be a sequence in the simple elective attractions. For example, there must be an error in the common tables of elective attractions, in which magnesia stands above ammonia under the sulfuric acid, and below it under the phosphoric, and the phosphoric acid stands above the sulfuric under magnesia, and below it under ammonia, since such an arrangement implies, that the order of the attractive forces is this; phosphate of magnesia, sulfate of magnesia, sulfate of ammonia, phosphate of ammonia, and again phosphate of magnesia; which forms a circle, and not a sequence. We must therefore either place magnesia above ammonia under the phosphoric acid, or the phosphoric acid below the sulfuric under magnesia; or we must abandon the principle of a numerical representation in this particular case.

In the second place, there must be an agreement between the simple and double elective attractions. Thus, if the fluoric acid stands above the nitric under barita, and below it under

lime, the fluate of barita cannot decompose the nitrate of lime, since the previous attractions of these two salts are respectively greater, than the divellent attractions of the nitrate of barita and the fluate of lime. Probably, therefore, we ought to place the fluoric acid below the nitric under barita; and we may suppose, that when the fluoric acid has appeared to form a precipitate with the nitrate of barita, there has been some fallacy in the experiment.

The third proposition is somewhat less obvious, but perhaps of greater utility: there must be a continued sequence in the order of double elective attractions; that is, between any two acids, we may place the different bases in such an order, that any two salts, resulting from their union, shall always decompose each other, unless each acid be united to the base nearest to it: for example, sulfuric acid, barita, potass, soda, ammonia, strontia, magnesia, glycina, alumina, zirconia, lime, phosphoric acid. The sulfate of potass decomposes the phosphate of barita, because the difference of the attractions of barita for the sulfuric and phosphoric acids is greater than the difference of the similar attractions of potass; and in the same manner the difference of the attractions of potass is greater than that of the attractions of soda; consequently the difference of the attractions of barita must be much greater than that of the attractions of soda, and the sulfate of soda must decompose the phosphate of barita: and in the same manner it may be shown, that each base must preserve its relations of priority or posteriority to every other in the series. It is also obvious that, for similar reasons, the acids may be arranged in a continued sequence between the different bases; and when all the decompositions of a certain number of salts

have been investigated, we may form two corresponding tables, one of the sequences of the bases with the acids, and another of those of the acids with the different bases; and if either or both of the tables are imperfect, their deficiencies may often be supplied, and their errors corrected, by a repeated comparison with each other.

In forming tables of this kind from the cases collected by FOURCROY, I have been obliged to reject some facts, which were evidently contradictory to others, and these I have not thought it necessary to mention; a few, which are positively related, and which are only inconsistent with the principle of numerical representation, I have mentioned in notes; but many others, which have been stated as merely probable, I have omitted without any notice. In the table of simple elective attractions, I have retained the usual order of the different substances; inserting again in parentheses such of them as require to be transposed, in order to avoid inconsequences in the simple attractions: I have attached to each combination marked with an asterisc the number deduced from the double decompositions, as expressive of its attractive force; and where the number is inconsistent with the corrected order of the simple elective attractions, I have also inclosed it in a parenthesis. Such an apparent inconsistency may perhaps in some cases be unavoidable, as it is possible that the different proportions of the masses concerned, in the operations of simple and compound decomposition, may sometimes cause a real difference in the comparative magnitude of the attractive forces. Those numbers, to which no asterisc is affixed, are merely inserted by interpolation, and they can only be so far employed for determining the mutual actions of the salts to which they belong,

as the results which they indicate would follow from the comparison of any other numbers, intermediate to the nearest of those, which are more correctly determined. I have not been able to obtain a sufficient number of facts relating to the metallic salts, to enable me to comprehend many of them in the tables.

It has been usual to distinguish the attractions, which produce the double decompositions of salts, into necessary and superfluous attractions; but the distinction is neither very accurate, nor very important: they might be still further divided, accordingly as two, three, or the whole of the four ingredients concerned are capable of simply decomposing the salt in which they are not contained; and if two, accordingly as they are previously united or separate; such divisions would however merely tend to divert the attention from the natural operation of the joint forces concerned.

It appears to be not improbable, that the attractive force of any two substances might, in many cases, be expressed by the quotient of two numbers appropriate to the substances, or rather by the excess of that quotient above unity; thus the attractive force of many of the acids for the three principal alkalies might probably be correctly represented in this manner; and where the order of attractions is different, perhaps the addition of a second, or of a second and third quotient, derived from a different series of numbers, would afford an accurate determination of the relative force of attraction, which would always be the weaker, as the two substances concerned stood nearer to each other in these orders of numbers; so that, by affixing, to each simple substance, two, three, or at most

four numbers only, its attractive powers might be expressed in the shortest and most general manner.

I have thought it necessary to make some alterations in the orthography generally adopted by chemists, not from a want of deference to their individual authority, but because it appears to me that there are certain rules of etymology, which no modern author has a right to set aside. According to the orthography universally established throughout the language, without any material exceptions, our mode of writing Greek words is always borrowed from the Romans, whose alphabet we have adopted: thus the Greek vowel υ, when alone, is always expressed in Latin and in English by Y, and the Greek diphthong ου by U, the Romans having no such diphthong as OU or OY. The French have sometimes deviated from this rule, and if it were excusable for any, it would be for them, since their *u* and *ou* are pronounced exactly as the υ and ου of the Greeks probably were: but we have no such excuse. Thus the French have used the term *acoustique*, which some English authors have converted into "acoustics;" our anatomists, however, speak, much more correctly, of the "acustic" nerve. Instead of glucine, we ought certainly, for a similar reason, to write glycine; or glycina, if the names of the earths are to end in *a*. Barytes, as a single Greek word, means weight, and must be pronounced bárytes; but as the name of a stone, accented on the second syllable, it must be written barites; and the pure earth may properly be called barita. Yttria I have altered to itria, because no Latin word begins with a Y.

Table of the Sequences of the Bases with the different Acids.

In all mixtures of the aqueous solutions of two salts, each acid remains united to the base which stands nearest to it in this table.

Sulfuric Acid.

Barita	Barita	Barita	Barita	Barita	Barita	Potass	Barita	Barita	Lead
Strontia	Strontia	Potass	Potass	Potass	Potass	Soda	Strontia	Potass	Mercury
Lime	Lime	Soda	Soda	Soda	Soda	Barita	Lime	Soda	Iron
(Silver ?)	Potass	Ammonia	Strontia	Strontia	Strontia	Strontia	Potass	Ammonia	Potass
(Mercury ?)	Soda	Strontia	Ammonia	Ammonia (4)	Ammonia (5)	Ammonia (6)	Soda	Strontia	Soda
Potass	(Mercury ?)	Magnesia (3)	Magnesia	Magnesia (4)	Magnesia	Lime	Magnesia ?	Magnesia	Magnesia
Soda	(Iron ?)	Glycina	Glycina	Glycina	Lime	Magnesia	Ammonia	Glycina	
Zinc, Iron, Copper	Magnesia	Alumina	Alumina	Alumina	Glycina	Glycina	Glycina	Alumina	Lead
Magnesia	Ammonia (2)	Zirconia	Zirconia	Zirconia	Alumina	Alumina	Alumina	Zirconia	Zinc, Copper
Ammonia (1)	Glycina	Lime	Lime	Lime	Zirconia	Zirconia	Zirconia	Lime ?	
Glycina	Alumina (2)								
Alumina	Zirconia								
Zirconia	(Copper ?)								
Nitric	Muriatic	Phosphoric	Fluoric	Sulfurous	Boracic	Carbonic	(Nitrous)	(Phosphorous)	(Acetic)

(1) Ammonia stands above magnesia when cold. (2) A triple salt is formed. (3) Perhaps magnesia ought to stand lower (4) A compound salt is formed, and when hot, magnesia stands above ammonia. (5) Fourcroy says, that sulfate of strontia is decomposed by borate of ammonia. (6) With heat, ammonia stands below lime and magnesia.

NITRIC ACID.

NITRIC AND MURIATIC ACIDS.

Barita	Potass	Barita	Potass	Barita (10)	Potass
Potass	Soda	Potass	Soda	Potass	Soda
Soda	Ammonia	Soda	Ammonia	Soda	Barita (10)
Strontia	Magnesia	Ammonia	Magnesia	Ammonia	Ammonia (7, 11)
Lime	Glycina	Magnesia	Glycina	Magnesia	Magnesia (7)
Magnesia (7)	Alumina	Glycina	Alumina	Glycina	Strontia
Ammonia (7)	Zirconia (8)	Alumina	Zirconia	Alumina	Lime
Glycina	Barita	Zirconia	Barita	Zirconia	Glycina
Alumina	Strontia	Strontia (9)	Strontia	Strontia	Alumina
Zirconia	Lime	Lime	Lime	Lime	Zirconia
MURIATIC	PHOSPHORIC	FLUORIC	SULFUROUS	BORACIC	CARBONIC

(7) A triple salt is formed. (8) FOURCROY says, that the muriate of zirconia decomposes the phosphates of barita and strontia. (9) According to FOURCROY's account, the fluate of strontia decomposes the muriates of ammonia, and of all the bases below it; but he says in another part of the same volume, that the fluate of strontia is an unknown salt. (10) According to FOURCROY's account of these combinations, barita should stand immediately below ammonia in both of these columns. (11) With heat, the carbonate of lime decomposes the muriate of ammonia.

PHOSPHORIC ACID.

Barita	Lime	Barita	Potass	Barita
Lime	Barita	Lime	Soda	Lime
Potass	Potass	Potass	Barita	Potass
Soda	Soda	Soda	Lime (13)	Soda
Strontia	Strontia	Strontia	Strontia	Strontia
Magnesia	Magnesia	Ammonia (12)	Ammonia	Magnesia
Ammonia	Ammonia	Magnesia	Magnesia	Glycina?
Glycina	Glycina	Glycina	Glycina	Alumina
Alumina	Alumina	Alumina	Alumina	Zirconia
Zirconia	Zirconia	Zirconia	Zirconia	
FLUORIC	SULFUROUS	BORACIC	CARBONIC	(PHOSPHOROUS)

(12) According to FOURCROY, the phosphate of ammonia decomposes the borate of magnesia. (13) FOURCROY says, that the carbonate of lime decomposes the phosphates of potass and of soda.

FLUORIC ACID.

Lime	Lime	Potass
Potass	Barita	Soda
Soda	Strontia	Lime
Magnesia	Potass	Barita
Ammonia	Soda	Strontia
Glycina	Ammonia	Ammonia (14)
Alumina	Magnesia	Magnesia
Zirconia	Glycina	Glycina
Strontia	Alumina	Alumina
Barita	Zirconia	Zirconia
SULFUROUS	BORACIC	CARBONIC

(14) According to FOURCROY, the carbonate of ammonia decomposes the fluates of barita and strontia.

Sulfurous Acid.				Boracic Acid.
Barita	Potass	Lime	Zirconia	Potass
Strontia	Soda	Strontia	Alumina	Soda
Potass	Barita (15)	Barita	Glycina	Lime
Soda	Strontia	Zirconia	Ammonia	Barita
Ammonia	Ammonia	Alumina	Magnesia	Strontia
Magnesia	Lime	Glycina	Strontia	Magnesia
Lime	Magnesia	Magnesia	Soda	Ammonia
Glycina	Glycina	Ammonia	Potass	Glycina
Alumina	Alumina	Soda	Barita	Alumina
Zirconia	Zirconia	Potass	Lime	Zirconia
Boracic	Carbonic	(Nitrous)	(Phosphorous?)	Carbonic

(15) Fourcroy says, that the sulfite of barita decomposes the carbonate of ammonia.

Table of the Sequences of the Acids with different Bases.

Barita.				Strontia.					Lime.					Potass / Soda / Magn.=Amm. / Glycina / Alumina / Zirconia — Each with every subsequent base in this order	Mag-nesia.
Sulfuric	S	C	S	S	C	S	P	S	C	P	P	P		S	B
Nitric	N	S	P	N	SS	P	S	P	P	F	F	F		N	C
Muriatic	M	P	SS	M	F	SS	SS	SS	F	B	B	SS		M	P
Phosphoric	SS	SS	N	SS	P	F	F	F	B	SS	C	S		P	F
Sulfurous	P	N	M	C	B	B	B	B	SS	S	SS	B		F	SS
Fluoric	C	M	F	B	S	C	C	N	S	C	S	N		SS	S
Boracic	B	F	B	F	M	N	N	M	M	N	N	M		B	N
Carbonic	F	B	C	P	N	M	M	C	N	M	M	C		C	M
Strontia	LM	PT	MG	LM	PT	MG	AM	GL	PT	MG	AM	GL			AM
		SD	AM		SD			AL	SD			AL			
			GL					ZR				ZR			
			AL												
			ZR												

The comparative use of this table may be understood from an example: if we suppose that the nitrate of barita decomposes the borate of ammonia, we must place the boracic acid above the nitric, between barita and ammonia in this table, and consequently barita below ammonia, between the fluoric and boracic in the former: hence the boracic and fluoric acids must also be transposed between barita and strontia, and between barita and potass; or if we place the fluoric still higher than the boracic in the first instance, we must place barita below ammonia between the nitric and fluoric acids, where indeed it is not impossible that it ought to stand.

Numerical Table of elective Attractions.

BARITA.		STRONTIA.		POTASS.	SODA.	LIMA.	
Sulfuric acid	1000°	Sulfuric acid	903°	Sulfuric acid		Oxalic acid	960
Oxalic	950	Phosphoric	827°		894° 885°	Sulfuric	868°
Succinic	930	Oxalic	825	Nitric	812° 804°	Tartaric	867
Fluoric		Tartaric	757	Muriatic	804° 797°	Succinic	866
Phosphoric	906°	*Fluoric*		Phosphoric		Phosphoric	865°
Mucic	900	Nitric	754°		801° 795°	Mucic	860
Nitric	849°	Muriatic	748°	Suberic?	745 740	Nitric	741°
Muriatic	840°	(Succinic)	740	Fluoric	671° 666°	Muriatic	736°
Suberic	800	(Fluoric)	703°	Oxalic	650 645	Suberic	735
Citric		*Succinic*		Tartaric	616 611	Fluoric	734°
Tartaric	760	Citric?	618	Arsenic	614 609	Arsenic	733½
Arsenic	733½	Lactic	603	Succinic	612 607	*Lactic*	732
(Citric)	730	*Sulfurous*	527°	Citric	610 605	Citric	731
Lactic	729	*Acetic*		Lactic	609 604	Malic	700
(Fluoric)	706°	*Arsenic*	(733½)	Benzoic	608 603	Benzoic	590
Benzoic	597	Boracic	513°	Sulfurous	488° 484°	*Acetic*	
Acetic	594	(Acetic)	480	Acetic	486 482	Boracic	537°
Boracic	(515)°	Nitrous?	430	Mucic	484 480	Sulfurous	516°
Sulfurous	592°	Carbonic	419°	Boracic	482° 479°	(Acetic)	470
Nitrous	450			Nitrous	440 437	Nitrous	425
Carbonic	420°			Carbonic	306° 304°	*Carbonic*	423°
Prussic	400			Prussic	300 298	Prussic	290

MAGNESIA.		AMMONIA.		GLYCINA?	ALUMINA.	ZIRCONIA?	
Oxalic acid	820	Sulfuric acid	808°	Sulfuric acid	718°	709°	700°
Phosphoric		Nitric	731°	Nitric	642°	634°	626°
Sulfuric	810°	Muriatic	729°	Muriatic	639°	632°	625°
(Phosphoric)	736°	Phosphoric	728°	Oxalic	600	594	588
Fluoric		Suberic?	720	Arsenic	580	575	570
Arsenic	733	Fluoric	613°	Suberic?	535	530	525
Mucic	732½	Oxalic	611	Fluoric	534°	529°	524°
Succinic	732½	Tartaric	609	Tartaric	520	515	510
Nitric	732°	Arsenic	607	Succinic	510	505	500
Muriatic	728°	Succinic	605	Mucic	425	420	415
Suberic?	700	Citric	603	Citric	415	410	405
(Fluoric)	620°	Lactic	601	*Phosphoric*	(648)°	(642)°	(636)°
Tartaric	618	Benzoic	599	Lactic	410	405	400
Citric	615	Sulfurous	433°	Benzoic	400	395	390
Malic?	600?	Acetic	432	Acetic	395	391	387
Lactic	573	Mucic	431	Boracic	388°	385°	382°
Benzoic	560	Boracic	430°	Sulfurous	355°	351°	347°
Acetic		Nitrous	400	Nitrous	340	336	332
Boracic	459°	Carbonic	339°	Carbonic	325°	323°	321°
Sulfurous	439°	Prussic	270	Prussic	260	258	256
(Acetic)	430						
Nitrous	410						
Carbonic	366°						
Prussic	280						

Acids.

Sulfuric.		Nitric.		Muriatic.		Phosphoric.	
Barita	1000°	Barita	849°	Barita	840°	Barita	906°
Strontia	903°	Potass	812°	Potass	804°	Strontia	827°
Potass	894°	Soda	804°	Soda	797°	*Lime*	(865)°
Soda	885°	Strontia	754°	Strontia	748°	Potass	801°
Lime	868°	Lime	741°	Lime	736°	Soda	795°
Magnesia	810°	Magnesia	732°	Ammonia	729°	*Ammonia*	(728)°
Ammonia	808°	Ammonia	731°	Magnesia	728°	Magnesia	736°
Glycina	718°	Glycina	642°	Glycina	639°	Glycina	648°
Itria	712	Alumina	634°	Alumina	632°	Alumina	642°
Alumina	709°	Zirconia	626°	Zirconia	625°	Zirconia	636°
Zirconia	700°						

Fluoric.		Oxalic.		Tartaric.		Arsenic.		Tungstic.	
Lime	734°	Lime	960		867	Lime	733½	Lime	
Barita	706°	Barita	950		760	Barita	733½	Barita	
Strontia	703°	Strontia	825		757	Strontia	733½	Strontia	
Magnesia	(620)°	Magnesia	820		618	Magnesia	733	Magnesia	
Potass	671°	Potass	650		616	Potass	614	Potass	
Soda	666°	Soda	645		611	Soda	609	Soda	
Ammonia	613°	Ammonia	611		609	Ammonia	607	Ammonia	
Glycina	534°	Glycina ?	600		520	Glycina	580	Glycina	
Alumina	529°	Alumina	594		515	Alumina	575	Alumina	
Zirconia	524°	Zirconia ?	588		510	Zirconia	570	Zirconia	

Succinic.		Suberic.		Camphoric.		Citric.	
Barita	930	Barita	800	Lime		Lime	731
Lime	866	Potass	745	Potass		Barita	730
Strontia ?	740	Soda	740	Soda		Strontia	618
(Magnesia)	732½	Lime	735	Barita		Magnesia	615
Potass	612	Ammonia	720	Ammonia		Potass	610
Soda	607	Magnesia	700	Glycina ?		Soda	605
Ammonia	605	Glycina ?	535 ?	Alumina		Ammonia	603
Magnesia		Alumina	530	Zirconia ?		Glycina ?	415 ?
Glycina ?	510	Zirconia ?	525 ?	Magnesia		Alumina	410
Alumina	505					Zirconia	405
Zirconia ?	500						

Lactic.		Benzoic.		Sulfurous.		Acetic.	
Barita	729	White oxid of arse-		Barita	592 °	Barita	594
Potass	609	nic		Lime	516 °	Potass	486
Soda	604	Potass	608	Potass	488 °	Soda	482
Strontia	603	Soda	603	Soda	484 °	*Strontia*	480
Lime	(732)	Ammonia	599	*Strontia*	(527)°	*Lime*	470
Ammonia	601	Barita	597	Magnesia	439 °	Ammonia	432
Magnesia	575	Lime	590	Ammonia	433 °	Magnesia	430
Metallic oxids		Magnesia	560	Glycina	355 °	Metallic oxids	
Glycina	410	Glycina ?	400 ?	Alumina	351 °	Glycina	395
Alumina	405	Alumina	395	Zirconia	347 °	Alumina	391
Zirconia	400	Zirconia ?	390 ?			Zirconia	387

MUCIC?	
Barita	900
Lime	860
Potass	484
Soda	480
Ammonia	431
Glycina	425
Alumina	420
Zirconia	415

BORACIC.	
Lime	537 °
Barita	515 °
Strontia	513 °
Magnesia	(459)°
Potass	482 °
Soda	479 °
Ammonia	430 °
Glycina	388 °
Alumina	385 °
Zirconia	382 °

NITROUS?	
Barita	450
Potass	440
Soda	437
Strontia	430
Lime	425
Magnesia	410
Ammonia	400
Glycina	340
Alumina	336
Zirconia	332

PHOSPHOROUS.	
Lime	
Barita	
Strontia	
Potass	
Soda	
Magnesia?	
Ammonia	
Glycina	
Alumina	
Zirconia	

CARBONIC.	
Barita	420 °
Strontia	419 °
Lime	(423)°
Potass?	306 °
Soda	304 °
Magnesia	(366)°
Ammonia	339 °
Glycina	325 °
Alumina	323 °
Zirconia	321 °

PRUSSIC.	
Barita	400
Strontia	
Potass	300
Soda	298
Lime	290
Magnesia	280
Ammonia	270
Glycina?	260
Alumina?	258
Zirconia?	256

VII. *Account of the Dissection of a Human Fœtus, in which the Circulation of the Blood was carried on without a Heart. By Mr.* B. C. Brodie. *Communicated by* Everard Home, *Esq. F. R. S.*

Read February 16, 1809.

An opportunity lately occurred to me of examining a human fœtus, in which the heart was wanting, and the circulation of the blood was carried on by the action of the vessels only. There have been some other instances of this remarkable deviation from the natural structure; but in that to which I allude the growth of the child had been natural, and it differed much less from the natural formation than in any of those, which are on record, and I have therefore been induced to draw up the following account of it.

A woman was delivered of twins in the beginning of the seventh month of pregnancy. There was a placenta with two umbilical chords, which had their origin about three inches distant from each other. The placenta was not preserved, but Mr. Adams, who attended the mother in her lying-in, observed nothing unusual in its appearance. Both fœtuses were born dead. They were nearly of the same size. One of them in no respect differed from the ordinary formation; the other had an unusual appearance, and Mr. Adams thought it deserving of examination. Through Dr. Hooper it was put into my hands for this purpose.

The fœtus measured thirteen inches from the summit of the cranium to the feet. The thorax and abdomen were surrounded by a large shapeless mass, which concealed the form of the whole upper part of the body. This mass proved to be the integuments covering the posterior part of the neck and thorax, distended with a watery fluid about three pints in quantity, contained in two cysts, lined by a smooth membrane. When the fluid was evacuated, and the cysts allowed to collapse, the fœtus had nearly the natural form. Its extremities had nearly the usual appearance, except that on the right hand there was no thumb; on the left hand there was no thumb also, and only a single finger. There were three toes on the right foot, and four toes on the left foot. The external nostrils consisted only of two folds of skin, under each of which was the orifice of an internal nostril, but pervious only for about half an inch. There was a hare lip, and a cleft in the bony palate extending one third of an inch backwards.

On dissection, the cranium was found somewhat compressed by the fluid contained in the cyst behind it. The brain itself was too putrid for accurate examination, but it was of nearly the natural size, and nothing unusual was observed in it. The membranes had the natural appearance, and the nerves appeared to go off from the brain and spinal marrow nearly as usual.

In the thorax there was no heart, thymus gland, or pleura. The trachea was situated immediately behind the sternum. It had its natural appearance, and divided as usual into the two bronchia. The latter terminated in the lungs, which consisted of two rounded bodies, not more than one third of an inch in diameter, having a smooth external surface, and

composed internally of a dense cellular substance. The œsophagus had the usual situation, but it terminated in a cul-de-sac at the lower part of the thorax. The rest of the thorax was filled with a dense cellular substance; and in place of the diaphragm, there was a membranous septum between it and the cavity of the abdomen.

In the abdomen, the stomach had no cardiac orifice. The intestine was attached to the mesentery in the usual way; but it was proportionably shorter than natural. There was an imperfect cœcum, but the colon was not distinguished by any difference of structure or appearance from the rest of the intestine. The rectum had its usual situation in the pelvis. The spleen and renal capsules were small; the kidnies, bladder, penis, and testicles had the usual appearance. The abdomen was lined by peritonæum, but there was no omentum. The liver and gall-bladder were wanting.

As there was no heart, it became an object of importance to ascertain the exact nature of the circulation: for this purpose, the blood-vessels were traced with attention.

The umbilical chord consisted of two vessels only: one of these was larger than the other, and its coats resembled those of a vein, while those of the smaller vessel were thick and elastic, like those of an artery. Both of these vessels entered the navel of the child. The artery passed to the left groin by the side of the urachus, occupying the usual situation of the left umbilical artery. Here it gave off the external and internal iliac arteries of the left side, and was then continued upwards on the fore-part of the spine forming the aorta. From the aorta arose the common trunk of the right iliac artery, and the branches to the viscera and parietes of the thorax and

abdomen. At the upper part of the thorax, it sent off the two subclavian, and afterwards divided into the two carotid arteries, without forming an arch. The veins corresponding to these arteries terminated in the vena cava, which was situated on the anterior part of the spine before the aorta, and passed downwards before the right kidney to the right groin. Here it became reflected upwards by the side of the urachus to the navel, and was continued into the larger vessel or vein of the chord.

It appears therefore, that, in this fœtus, not only the heart was wanting, but there was no communication of any kind between the trunks of the venous and arterial systems, as in the natural fœtus, where there is a heart. The only communication between the two sets of vessels, was by means of the capillary branches anastomosing as usual in the fœtus and in the placenta. The blood must have been propelled from the placenta to the child through the artery of the chord, and must have been returned to the placenta by means of the vein, so that the placenta must have been at once the source and the termination of the circulation, and the blood must have been propelled by the action of the vessels only.

It is to be understood, that the circulation in the fœtus receives no propelling power from the action of the heart and arteries of the mother. This, although perfectly known to anatomists, it is proper to mention, as it may not be equally known to all the members of this Society.

It appears extraordinary, that under these circumstances, notwithstanding the circulation through the placenta must have been more languid than is natural, that organ should nevertheless have been capable of exercising its proper func-

tions, so as to produce those changes on the blood, which are necessary for the maintenance of fœtal life. This may be explained by considering that in the natural fœtus the umbilical arteries are branches of the general arterial system, and only a portion of the blood of the child is sent to the placenta, whereas in the fœtus which I have described, the trunk of the vena cava was continued into the vein of the chord, and the whole of the venous blood circulated through the placenta, and was exposed to the influence of the arterial blood of the mother.

But the most interesting circumstance, which we learn from this examination is, that the circulation not only can be carried on without a heart, but that a child so circumstanced can be maintained in its growth, so as to attain the same size as a fœtus which is possessed of that organ. This fact is contrary to what prior experience has led us to expect, as will appear from the following abstract of the authenticated cases of this species of malformation, which we find on record.

A monster, in which there was no heart, is described by M. Mery.* There were twins, one of which was well formed, and of the usual size of a six month's child: the size of the other was not mentioned, so that no comparison could be made between them. In the latter, the head, neck, and upper extremities were wanting. There were no vestiges of a brain, nor was there any liver. The dissection of the blood-vessels does not appear to have been very accurately made, but from the general account I should suppose, that the circulation did not materially differ from that of the fœtus which I have described.

* *Histoire de l'Academie Royale des Sciences*, 1720.

Another instance of this kind is described by M. WINSLOW.* This was also a twin, only seven inches in length. The age and size of the other child are not mentioned. In this instance there was no head, nor any vestige of brain. There were no lungs, liver, stomach, nor spleen, and only a small portion of intestine. The arterial system is described as being complete, communicating with the placenta by the umbilical vein opening into the aorta, and the umbilical arteries arising nearly as usual. In this instance there was a circle of vessels formed by the arteries only, for M. WINSLOW expressly states, that there were no veins; and however extraordinary this may appear, we cannot be otherwise than cautious in denying an observation made by an anatomist, so remarkable for his extreme accuracy and minuteness.

Dr. LE CAT of Rouen, states another case of twins† born at the end of the ninth month of pregnancy. One of them was a well formed child of the usual size; but the other was only twelve inches and a half in length. The head of the latter was very imperfect, and there was only a very minute portion of brain. The heart, lungs, liver, stomach, and spleen were entirely wanting, and there was only a small portion of intestine. The arterial system was perfect; the umbilical vein terminated in the aorta, and the umbilical arteries had their origin from the internal iliac, as usual. There is, however, an obscurity in the account of the circulation, as it is stated that there were veins, but they were not traced, nor was any communication made out between them and the arteries, or the vessels of the chord.

* *Histoire de l'Academie Royale des Sciences*, 1740.

† Phil. Trans. for 1767.

Dr. CLARKE* has given an account of a case, in which a woman, after a natural labour, was delivered of a healthy child, and also of a substance covered by common integuments, of an oval form, four inches in length, and having a separate navel string and placenta. In this substance there was one os innominatum, with a femur, tibia, and fibula. There were neither brain nor nerves; nor were there any viscera, except a small portion of intestine. The umbilical chord consisted of two vessels, an artery and a vein, both of which ramified in this substance and in the placenta.

In Dr. HUNTER's anatomical collection, there are two specimens of monsters born without hearts. In both of them the whole upper part of the body was wanting; and in neither was the exact nature of the circulation ascertained.

In each of the instances which I have quoted, not only the heart was wanting, but the fœtus in other respects was so imperfect, that it could not be considered as any thing more than a mola, or an irregularly formed living mass connected with the placenta. In particular, in all of them the brain, which may with justice be considered as affording the best distinction between a mola and a fœtus, was wanting; whereas in that which forms the subject of the present paper, the brain was nearly as large as usual, and in other respects the fœtus varied much less from the natural structure, than in any former instance.

In the cases already on record, we have seen, that wherever the size of the monster was mentioned, it was much smaller than a natural fœtus. This would have led to the supposition, that a circulation, which was carried on by the action of the vessels only, was incapable of maintaining the

* Phil. Trans. for 1793.

natural growth of a child, had it not been found that the fœtus, which I have described, though the heart was wanting, was fully equal in size to a fœtus of the same age, which was possessed of that organ.

It may be observed, that in all these cases, in which the heart was wanting, the liver was wanting also. It is probable, that the action of the vessels only, without the assistance of the heart, would have been insufficient to propel the blood through the circulation of the liver, which is so extensive in the natural fœtus.

VIII. *On the Origin and Formation of Roots. In a Letter from T. A. Knight, Esq. F. R. S. to the Right Hon. Sir Joseph Banks, K. B. P. R. S.*

Read February 23, 1809.

My dear Sir,

In a former communication I have given an account of some experiments, which induced me to conclude that the buds of trees invariably spring from their alburnum, to which they are always connected by central vessels of greater or less length; and in the course of much subsequent experience, I have not found any reason to change the opinion that I have there given.* The object of the present communication is to shew, that the roots of trees are always generated by the vessels which pass from the cotyledons of the seed, and from the leaves, through the leaf-stalks and the bark, and that they never, under any circumstances, spring immediately from the alburnum.

The organ, which naturalists have called the radicle in the seed, is generally supposed to be analogous to the root of the plant, and to become a perfect root during germination; and I do not know that this opinion has ever been controverted, though I believe that, when closely investigated, it will prove to be founded in error.

A root, in all cases with which I am acquainted, elongates only by new parts which are successively added to its apex or

* Phil. Trans. 1805.

point, and never, like the stem or branch, by the extension of parts previously organized; and I have endeavoured to shew, in a former memoir, that owing to this difference in the mode of the growth of the root and lengthened plumule of germinating seeds, the one must ever be obedient to gravitation, and point towards the centre of the earth, whilst the other must take the opposite direction.* But the radicle of germinating seeds elongates by the extension of parts previously organised, and in a great number of cases, which must be familiar to every person's observation, raises the cotyledons out of the mould in which the seed is placed to vegetate. The mode of growth of the radicle is therefore similar to that of the substance which occupies the spaces between the buds near the point of the succulent annual shoot, and totally different from that of the proper root of the plant, which I conceive to come first into existence during the germination of the seed, and to spring from the point of what is called the radicle. At this period, neither the radicle nor cotyledons contain any alburnum; and therefore the first root cannot originate from that substance; but the cortical vessels are then filled with sap, and apparently in full action, and through these the sap appears to descend which gives existence to the true root.

When first emitted, the root consists only of a cellular substance, similar to that of the bark of other parts of the future tree, and within this the cortical vessels are subsequently generated in a circle, inclosing within it a small portion of the cellular substance, which forms the pith or medulla of the root. The cortical vessels soon enter on their office of gene-

* Phil. Trans. 1806.

rating alburnous matter; and a transverse section of the root then shews the alburnum arranged in the form of wedges round the medulla, as it is subsequently deposited on the central vessels of the succulent annual shoot, and on the surface of the alburnum of the stems and branches of older trees.*

If a leaf-stalk be deeply wounded, a cellular substance, similar to that of the bark and young root is protruded from the upper lip of the wound, but never from the lower; and the leaf-stalks of many plants possess the power of emitting roots, which power can not have resided in alburnum, for the leaf-stalk does not contain any: but vessels, similar to those of the bark and radicle, abound in it, and apparently convey the returning sap; and from these vessels, or perhaps more properly from the fluid they convey, the roots emitted by the leaf-stalk derive their existence.†

If a portion of the bark of a vine, or other tree, which readily emits roots, be taken off in a circle extending round its stem, so as to intercept entirely the passage of any fluid through the bark; and any body which contains much moisture be applied, numerous roots will soon be emitted into it immediately above the decorticated space, but never immediately beneath it: and when the alburnum in the decorticated spaces has become lifeless to a considerable depth, buds are usually protruded beneath, but never immediately above it, apparently owing to the obstruction of the ascending sap. The roots, which are emitted in the preceding case, do not appear in any degree to differ from those which descend from the radicles of generating seeds, and both apparently derive

* Phil. Trans. for 1801, Plate 27.

† Phil. Trans. for 1803.

their matter from the fluid which descends through the cortical vessels.

There are several varieties of the apple tree, the trunks and branches of which are almost covered with rough excrescences, formed by congeries of points which would have become roots under favourable circumstances; and such varieties are always very readily propagated by cuttings. Having thus obtained a considerable number of plants of one of these varieties, the excrescences began to form upon their stems when two years old, and mould being then applied to them in the spring, numerous roots were emitted into it early in the summer. The mould was at the same time raised round, and applied to, the stems of other trees of the same age and variety, and in every respect similar, except that the tops of the latter were cut off a short distance above the lowest excrescence, so that there were no buds or leaves from which sap could descend to generate or feed new roots; and under these circumstances no roots, but numerous buds were emitted, and these buds all sprang from the spaces and points, which under different circumstances had afforded roots. The tops of the trees last mentioned, having been divided into pieces of ten inches long, were planted as cuttings, and roots were by these emitted from the lowest excrescences beneath the soil, and buds from the uppermost of those above it.

I had anticipated the result of each of the preceding experiments; not that I supposed, or now suppose, that roots can be changed into buds, or buds into roots; but I had before proved that the organization of the alburnum is better calculated to carry the sap it contains, from the root upwards, than in any other direction, and I concluded that the sap when

arrived at the top of the cutting through the alburnum would be there employed, as I had observed in many similar cases, in generating buds, and that these buds would be protruded where the bark was young and thin, and consequently afforded little resistance.* I had also proved the bark to be better calculated to carry the sap towards the roots than in the opposite direction, and I thence inferred that as soon as any buds, emitted by the cuttings, afforded leaves, the sap would be conveyed from these to the lower extremity of the cuttings by the cortical vessels, and be there employed in the formation of roots.†

Both the alburnum and bark of trees evidently contain their true sap; but whether the fluid which ascends in such cases as the preceding through the alburnum to generate buds, be essentially different from that which descends down the bark to generate roots, it is perhaps impossible to decide. As nature, however, appears in the vegetable world to operate by the simplest means; and as the vegetable sap, like the animal blood, is probably filled with particles which are endued with life, were I to offer a conjecture, I am much more disposed to believe that the same fluid, even by merely acquiring different motions, may generate different organs, than that two distinct fluids are employed to form the root, and the bud and leaf.

When alburnum is formed in the root, that organ possesses, in common with the stem and branches, the power of producing buds, and of emitting fibrous roots, and when it is detached from the tree, the buds always spring near its upper end, and the roots near the opposite extremity, as in the

* Phil. Trans for 1805.

cuttings abovementioned. The alburnum of the root is also similar to that of other parts of the tree, except that it is more porous, probably owing to the presence of abundant moisture during the period in which it is deposited.* And possibly the same cause may retain the wood of the root permanently in the state of alburnum; for I have shewn, in a former memoir, that if the mould be taken away, so that the parts of the larger roots, which adjoin the trunk, be exposed to the air, such parts are subsequently found to contain much heart wood.*

I would wish the preceding observations to be considered as extending to trees only, and exclusive of the palm tribe; but I believe they are nevertheless generally applicable to perennial herbaceous plants, and that the buds and fibrous roots of these originate from substances which correspond with the alburnum and bark of trees. It is obvious, that the roots which bulbs emit in the spring, are generated by the sap which descends from the bulb, when that retains its natural position; and such tuberous rooted plants as the potatoe offer rather a seeming than a real obstacle to the hypothesis I am endeavouring to establish. The buds of these are generally formed beneath the soil; but I have shewn, in a former memoir, that the buds on every part of the stem may be made to generate tubers, which are similar to those usually formed beneath the soil; and I have subsequently seen, in many instances, such emitted by a re-produced bud, without the calix of a blossom, which had failed to produce fruit; but I have never, under any circumstances, been able to obtain tubers from the fibrous roots of the plant.

The tube therefore appears to differ little from a branch,

* Phil. Trans. for 1801.

which has dilated instead of extending itself, except that it becomes capable of retaining life during a longer period; and when I have laboured through a whole summer to counteract the natural habits of the plant; a profusion of blossoms has in many instances sprung from the buds of a tuber.

The runners also, which, according to the natural habit of the plant, give existence to the tubers beneath the soil, are very similar in organization to the stem of the plant, and readily emit leaves and become converted into perfect stems, in a few days, if the current of ascending sap be diverted into them; and the mode in which the tuber is formed above, and beneath the soil, is precisely the same. And when the sap, which has been deposited at rest during the autumn and winter, is again called into action to feed the buds, which elongate into parts of the stems of the future plants in the spring, fibrous roots are emitted from the bases of these stems, whilst buds are generated at the opposite extremities, as in the cases I have mentioned respecting trees.

Many naturalists* have supposed the fibrous roots of all plants to be of annual duration only; and those of bulbous and tuberous rooted plants certainly are so: as in these nature has provided a distinct reservoir for the sap which is to form the first leaves and fibrous roots of the succeeding season; but the organization of trees is very different, and the alburnum and bark of the roots and stems of these are the reservoirs of their sap during the winter.† When, however, the fibrous roots of trees are crowded together in a garden-pot, they are often found lifeless in the succeeding spring; but I

* M. MIRBEL's *Traité d'Anatomie*, &c. &c. Dr. SMITH's Introduction to Botany.

† Phil. Trans. for 1805.

have not observed the same mortality to occur, in any degree, in the roots of trees when growing, under favourable circumstances, in their natural situation.

I am prepared to offer some observations on the causes which direct the roots of plants in search of proper nutriment and which occasion the root of the same plant to assume different forms under different circumstances; but I propose to make those observations the subject of a future communication.

I am, MY DEAR SIR,

with great respect,

your much obliged, &c. &c.

THOMAS AND. KNIGHT.

Elton, Dec. 22, 1808.

IX. *On the Nature of the intervertebral Substance in Fish and Quadrupeds.* *By* Everard Home, *Esq. F. R. S.*

Read February 23, 1809.

In examining the internal structure of a Squalus maximus of Linnæus, that lately came under my observation, a description of which will be the subject of a future paper, I met with a peculiarity in the intervertebral substance of the spine not hitherto made known to the public.

The fish is thirty feet six inches long, the diameter of the larger vertebræ near the head, seven inches. The intervertebral substance was cut into by Mr. Clift four days after the fish was brought on shore, and a limpid fluid rushed out with so much velocity, that it rose to the height of four feet.

At the end of twelve days, I had an opportunity of examining a portion of the spine, the intervertebral joints of which were preserved entire; upon sawing through two of the vertebræ, a fluid was met with, of the consistence of liquid jelly with clots of different sizes floating in it, so that in eight days a considerable tendency to coagulation had taken place, although the fluid was entirely excluded from the external air.

The form of the cavity thus exposed by a longitudinal section being made of it, is nearly spherical, capable of containing three pints of liquid, the lateral parts are ligamentous and elastic, uniting together the edges of the concave surfaces of

the two contiguous vertebræ. When the liquid is evacuated, the elasticity of the lateral ligaments brings the ends of the vertebræ within an inch and half of each other; in this state the inner layers of the ligaments, which are less firm in texture than the outer, project into the cavity, and may be mistaken for a part of its natural contents; this portion when soaked in water swells out to a considerable size, the water readily insinuating itself between the membranous layers of which it is composed.

The whole thickness of the lateral ligaments is about one inch, the external half of which is compact and elastic, the other appears to possess a very slight degree of elasticity. The appearance of the joint is shewn in the annexed drawing, and an account of the analysis of the fluid by Mr. W. BRANDE forms a postscript to this paper. Every part of the mechanism is formed upon so large a scale, that it is rendered conspicuous, and nothing is left to doubt or conjecture; the nature of the joint is different from every other that is met with in animal bodies, and there are many circumstances respecting it, which render it uncertain whether human ingenuity can ever make any resemblance to it, that can be applied to the purposes of mechanics.

These would have been sufficient grounds for bringing this subject before the Society; but there are others of still greater importance which have induced me to make it a separate communication; it enables us to explain the general principle upon which all intervertebral joints are formed, which has been hitherto but imperfectly understood. With this view, I will first describe the principle upon which this particular joint is formed, and then shew the resemblance that it bears to

those of other animals, in which the parts are not so readily distinguished from one another, and consequently their precise use has not been accurately ascertained.

The fluid contained in the cavity being incompressible, preserves a proper interval between the vertebræ to allow of the play of the lateral elastic ligaments, and forms a ball round which the concave surfaces of the vertebræ are moved, and readily adapts itself to every change which takes place in the form of the cavity.

The elasticity of the ligaments, by its constant action, renders the joint always firm, independent of any other support, and keeps the ends of the vertebræ opposed to each other, so that the whole spine is preserved in a straight line, unless it is acted on by muscles or some other power. When a muscular force is applied to one side of the spine, it stretches the elastic ligament on the opposite side of the joint, and as soon as that force ceases to act, the joint returns to the former state. This is one of the most beautiful instances in nature of elasticity being employed as a substitute for muscular action.

The extent of the motion in each particular joint is undoubtedly small, but this is compensated by their number, and the elasticity of the vertebræ themselves.

Fish in general have their vertebræ formed with similar concavities to those of the squalus maximus; these, when examined after death, contain a solid jelly, but in the living fish it is found in a fluid state. This fact was ascertained in the skate, the smallness of the quantity of fluid in any one joint, and the readiness with which it coagulates after death, prevented it from being before observed: the fluid in the

skate is found by Mr. W. BRANDE to have the same properties, as far as the small quantity that can be collected admits of examination, with that in the squalus maximus.

Although this structure of the intervertebral joint appears to be common to fish in general; the form of the cavity is not in all exactly the same; in the skate it is very similar to that in the squali, but in the common eel, it is more oblong, the longitudinal diameter being about one third longer than the transverse one.

It is evidently contrived for producing the quick vibratory lateral motion, which is peculiar to the back bones of fish while swimming, and enables them to continue that motion for a great length of time, with a small degree of muscular action.

In the sturgeon, there are some curious peculiarities in the structure of the spine. Externally there is the common appearance of regular vertebræ, but these prove to be only cartilaginous rings, the edges of which are nearly in contact, and are united together by elastic ligaments, forming a tube the whole length of the spine, this is lined throughout its internal surface with a firm compact elastic substance, about the thickness of the cartilaginous tube, within this is a soft flexible substance in a small degree elastic; in the centre there is a chain of cavities in the form of lozenges, containing a fluid, and communicating with one another by very small apertures bearing a slight similarity to the intervertebral cavities of the spine in other fish.

As all the different parts of which this spine is composed are more or less elastic, except the central fluid, it must have great flexibility adapting it to the motions of this particular

fish. The structure of the spine in the lamprey eel resembles that of the sturgeon.

The intervertebral joint which is common to fish, is not met with in any of the whale tribe, whose motion through the water is principally effected by means of their horizontal tail; in them the substance employed to unite the vertebræ together is the same as in quadrupeds in general, and from the size of the vertebræ it is on a larger scale, and rendered more conspicuous.

The external portion is very firm and compact, is ranged in concentric circles with transverse fibres uniting the layers together, it becomes softer towards the middle, and in the centre there is a pliant soft substance without elasticity, but admitting of extension more like a jelly than an organized body, corresponding in its use to the incompressible fluid in the fish.

To ascertain whether this structure was generally met with in the spines of quadrupeds, Mr. BRODIE, at my request, examined the intervertebral substance in a great many animals, and found, what, undoubtedly, was very little to be expected, that in the hog and rabbit, in the central part, there is a cavity with a smooth internal surface of the extent of half the diameter of the vertebra, in which is contained a thick gelatinous fluid, so that in some quadrupeds there is an approach towards the intervertebral joint in fish; but whether this is to answer any essential purpose to these animals, or is only to form an intermediate link in the chain of gradation of structures, which is so uniformly adhered to in the productions of nature, cannot at present be determined.

In the bullock, sheep, deer, monkey, and man, the struc-

ture corresponds with that of the whale; in the three last, the central substance appears to be the most compact. Besides the structures already mentioned, there is in some animals one of a very different kind; in the alligator the vertebræ through the whole length of the spine, have regular joints between them, the surfaces are covered with articulating cartilages; and there is synovia and a capsular ligament. In the snake, there is a regular ball and socket joint between every two vertebræ; so that the means employed for the motion of the back bone in different animals, comprehends almost every species of joint with which we are acquainted.

Having mentioned a sufficient number of facts to point out the animals, in which the different structures of the intervertebral substance are to be found, I have abstained from being more particular in my account; as it would in no respect elucidate the principal object of the present communication.

From the facts and observations which have been stated, it appears that the intervertebral substance of the human spine does not consist entirely of elastic ligament, dense in its texture at the circumference, and becoming gradually softer towards the centre; but that the middle portion is composed of materials which render it very pliant, though not at all elastic, fitting it to keep the vertebræ at the proper distance from each other, so as to admit of the action of the lateral elastic ligaments.

When this knowledge is applied to the treatment of curvatures of the spine, a complaint so commonly met with in young women, whose strength does not bear the necessary proportion to the growth of the body, it will show the great impropriety of overstretching the intervertebral ligaments,

since in that state the central substance no longer supports the vertebræ, and the joints must lose their proper firmness, which will be attended with many disadvantages.

As the principal motive which induces me to prosecute the laborious researches of comparative anatomy, is to attain a more complete knowledge of the structure and functions of the human body, than can be acquired in any other way, and to apply that knowledge to the most useful of all purposes, the cure of diseases, the success which has attended my labours, in the present instance, affords me particular satisfaction; it encourages me in the pursuit of those inquiries, and holds out an invitation to others, by showing them that the paths of nature, however frequently they have been traced, are not yet sufficiently explored.

EXPLANATION OF THE PLATE.

A longitudinal section of one of the intervertebral joints of the squalus maximus, after the fluid had been evacuated, and the parts had been steeped in water.

a a a a. The section of the vertebra to show its shape and the two concave surfaces which form the intervertebral cavities. The vertebra itself is partly bone, and partly transparent cartilage; the bony portion forms the two cup-like cavities, and the intermediate substance consists of bony cells in form of lozenges filled with cartilage.

The cavity of the joint is in its contracted state, and the inner portion of the lateral ligaments, which is made up of thin layers of a loose texture, has its interstices loaded with water,

which makes it project into the cavity of the joint more than it could do in a natural state.

The external portion of the ligament, to the thickness of half an inch, is the only truly elastic part on which its strength depends.

A chemical Analysis of the Fluid contained in the intervertebral Cavity of the Squalus maximus. *By Mr.* William Brande.

The fluid found in the intervertebral cavities is of an opal colour; it is semi-transparent, and has a strong fishy smell and taste.

Its specific gravity is ,1027.

In the first instance it does not readily mix with water; but is easily diffused through that fluid by agitation.

When heated in a water bath to a temperature of 212°, it becomes more transparent, but undergoes no farther apparent change.

Infusion of galls and of catechu produce no alteration in it.

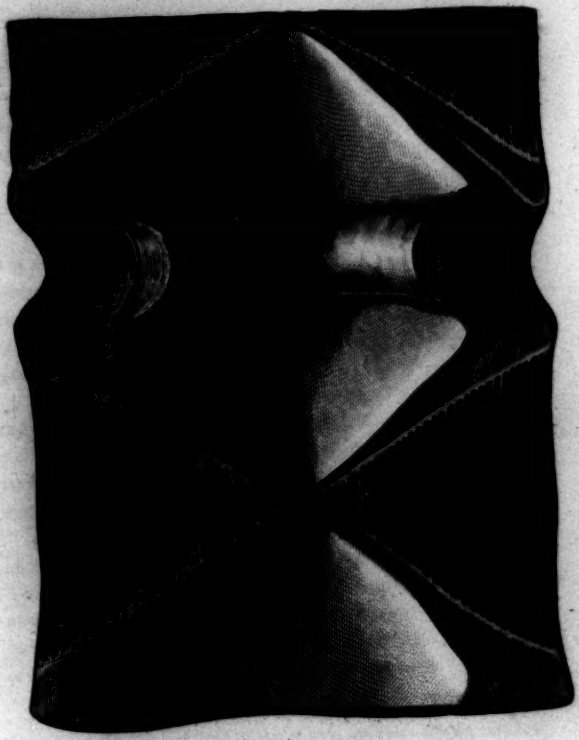
Solution of oxymuriat of mercury occasioned a very copious white precipitate, and a similar effect was produced by a solution of nitro-muriat of tin.

Nitrat of silver and acetat of lead threw down precipitates of muriat of silver and of lead.

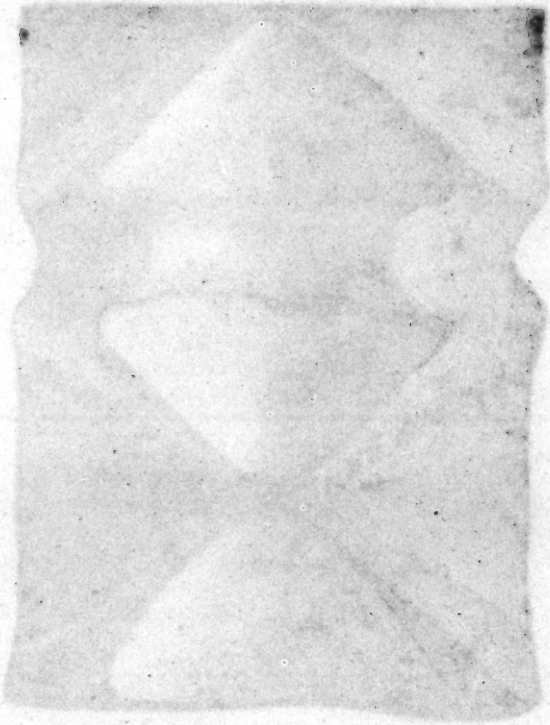
Muriatic acid occasioned a slight cloud after two hours had elapsed, and after twenty-four hours, a small quantity of white flaky matter separated.

Alcohol produced no change.

The fluid readily mixed with a solution of pure potash, a



Scale two thirds of an Inch to one Inch.



small quantity of ammonia being at the same time evolved. Muriatic acid did not produce any immediate precipitation in this alkaline solution.

The effect of these re-agents, evidently proves the non-existence of gelatine in this fluid; it would also appear that it contains no albumen, unless the effects produced by muriatic acid and by the oxymuriat of mercury and of tin, be regarded as indications of that substance.

It seems to approach nearer to *mucus* or *mucilage*, than to any other animal fluid.*

When the fluid is evaporated in a temperature not exceeding 220° to half its bulk, an opaque substance in the form of bluish white filaments, gradually separates. A thin semi-transparent pellicle forms at the same time upon the surface, which, when removed, is soon succeeded by another. These pellicles were dried on bibulous paper.

The fluid part, remaining after the separation of the filamentous substance and pellicles, afforded a very distinct yellowish cloud, with solutions containing tannin. It was somewhat turbid, but did not form any deposit. In other respects, it nearly resembled the original fluid before evaporation.

The filaments which appeared during evaporation, were separated by passing the fluid through a piece of fine muslin. They resembled albumen imperfectly coagulated, not only in appearance, but in most of their chemical properties.

When the fluid began to putrify, a considerable quantity of the same substance separated spontaneously.

* By *mucus of animals*, I mean a glary fluid, which does not mix readily with water, which is neither coagulated by heat or acids, and which does not form a precipitate with solutions containing tannin.

This substance was insoluble in water, and when boiled for a few minutes in that fluid, it became whiter, harder, and more opaque.

It underwent the same change in alcohol, and when boiled in alcohol, or in dilute muriatic acid, it became still more firm, and appeared like perfectly coagulated albumen.

In this state it was soluble in a solution of pure potash, forming a saponaceous compound, which was decomposed by dilute muriatic acid, a white flaky precipitate being formed. It possessed the other properties which Mr. HATCHETT has enumerated as belonging to coagulated albumen.*

When the pellicle, which had formed on the surface of the fluid during evaporation, was nearly dry, it became somewhat tough and elastic; it was semi-transparent, and of a dirty white colour.

When boiled for some time in water, about three fourths of it were found to be soluble in that fluid, the remainder, when separated by filtration, possessed the properties of the albuminous substance already mentioned.

The solution afforded a copious precipitate with solutions containing tannin. It was not at first affected, either by oxy-muriat of mercury, or of tin; but after twenty four hours, a slight deposit took place.

Although these re-agents indicated the presence of a substance having the properties of pure gelatine in solution, yet it could not be brought to gelatinize by the usual method of evaporation.

From these experiments it would appear, that the intervertebral fluid, is of a peculiar nature; that in its original

* Vide Phil. Trans. 1800.

properties it resembles mucus, but that under certain circumstances it is capable of being converted into modifications of gelatine and albumen.

The intervertebral fluid of the skate was found to resemble mucus; it did not exhibit any traces of albumen, but the quantity which I procured for examination being very small, I was unable to ascertain its further analogies to the fluid found in the intervertebral cavities of the Squalus maximus.

From the Press of
W. BULMER & Co.
Cleveland-Row, St. James's,
London.

properties it resembles mucus, but that under certain circumstances it is capable of being converted into modifications of gelatine and albumen.

The intervertebral fluid of the skate was found to resemble mucus; it did not exhibit any traces of albumen, but the quantity which I procured for examination being very small, I was unable to ascertain its further analogies to the fluid found in the intervertebral cavities of the Squalus maximus.

From the Press of
R. BULMER & Co.
Cleveland Row, St. James's,
London.

METEOROLOGICAL JOURNAL,

KEPT AT THE APARTMENTS

OF THE

ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

MDCCCIX.

METEOROLOGICAL JOURNAL
for January, 1808.

1808	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Jan. 1	43	8	0	43	50	29,20	66	0,095	S	2	Rain.
	47	2	0	47	53	29,23	63		S	2	Fair.
2	43	8	0	45	51	28,90	68	0,155	S	2	Rain.
	46	2	0	44	53	29,03	60		WSW	2	Fair.
3	34	8	0	34	51	29,25	66	0,233	WSW	1	Fine.
	42	2	0	42	53	29,35	60		WSW	1	Cloudy.
4	36	8	0	34	51	29,93	63		W	1	Cloudy.
	39	2	0	39	53	29,98	61		S	1	Fine.
5	39	8	0	46	51	29,80	73		SSW	1	Cloudy.
	50	2	0	49	53	29,68	74		S	1	Rain.
6	41	8	0	41	52	30,25	64		SSW	1	Cloudy.
	48	2	0	48	53	30,34	62		WNW	1	Cloudy.
7	42	8	0	44	53	30,46	70		SSW	1	Cloudy.
	49	2	0	49	55	30,47	72		SSW	1	Cloudy.
8	44	8	0	45	54	30,51	74		W	1	Foggy.
	46	2	0	46	56	30,51	71		W	1	Cloudy.
9	40	8	0	41	53	30,50	74		W	1	Foggy.
	47	2	0	47	55	30,50	71		NW	1	Cloudy.
10	44	8	0	44	53	30,43	73		W	1	Cloudy.
	49	2	0	47	56	30,27	67		SW	1	Cloudy.
11	46	8	0	48	54	29,95	70		WNW	1	Cloudy.
	47	2	0	47	56	29,98	58		NW	1	Cloudy.
12	37	8	0	37	53	29,92	63		N	1	Fair.
	39	2	0	39	55	29,90	58		NW	1	Fair.
13	35	8	0	37	53	29,78	65		W	1	Cloudy.
	46	2	0	44	56	29,80	63		WNW	1	Fair.
14	41	8	0	43	53	29,20	65	0,072	WSW	1	Cloudy.
	47	2	0	47	56	29,12	64		W	1	Rain.
15	30	8	0	30	50	29,96	55	0,020	N	2	Fine.
	34	2	0	34	52	30,07	53		N	2	Fine.
16	20	8	0	20	48	30,16	62		N	1	Cloudy.
	33	2	0	32	50	30,12	60		NW	1	Cloudy.

METEOROLOGICAL JOURNAL
for January, 1808.

1808	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.		Inches.			
Jan. 17	26	8	0	26	48	30,35	63		NE	1	Fine.
	36	2	0	36	51	30,43	58		NE	1	Fair.
18	27	8	0	27	47	30,48	65		NE	1	Fair.
	35	2	0	35	49	30,44	63		N	1	Fair.
19	29	8	0	35	47	30,22	66		WNW	1	Foggy.
	41	2	0	40	50	30,10	67		WSW	1	Cloudy.
20	38	8	0	38	48	29,65	70	0,063	W	1	Cloudy.
	41	2	0	41	51	29,60	62		NW	1	Fair.
21	23	8	0	23	47	29,82	58		NNE	1	Cloudy.
	30	2	0	37	49	29,91	60		NW	1	Cloudy.
22	18	8	0	18	46	30,17	60		W	1	Cloudy.
	33	2	0	27	48	30,17	55		W	1	Fair.
23	23	8	0	31	46	30,04	63		SW	1	Fair.
	40	2	0	38	49	29,94	67		SSW	1	Cloudy.
24	36	8	0	36	47	29,83	73	0,016	SW	1	Foggy.
	40	2	0	40	50	29,75	73		SW	1	Cloudy.
25	34	8	0	34	47	29,42	73	0,280	SW	1	Cloudy.
	40	2	0	38	49	29,34	72		SW	1	Cloudy.
26	28	8	0	28	47	29,20	68		SE	1	Cloudy.
	34	2	0	33	49	29,28	63		W	1	Fair.
27	26	8	0	27	46	29,51	65		WNW	1	Fair.
	42	2	0	37	51	29,52	62		SW	1	Fair.
28	37	8	0	47	47	29,22	70		SW	2	Cloudy.
	48	2	0	46	51	29,25	58		W	2	Cloudy.
29	36	8	0	36	49	29,51	61		W	2	Fine.
	43	2	0	43	52	29,64	55		WNW	2	Fine.
30	40	8	0	41	50	29,73	67	0,080	SW	1	Rain.
	51	2	0	50	53	29,64	72		SW	1	Cloudy.
31	49	8	0	49	51	29,69	73	0,045	SW	2	Cloudy.
	53	2	0	52	56	29,77	64		W	2	Cloudy.

METEOROLOGICAL JOURNAL
for February, 1808.

1808	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Feb. 1	47	7	0	48	52	29,86	73		SSW	1	Cloudy.
	53	2	0	52	56	29,87	67		SW	1	Cloudy.
2	48	7	0	48	55	29,64	65		SSW	2	Cloudy.
	53	2	0	50	57	29,57	60		SSW	2	Rain.
3	40	7	0	40	53	29,77	62	0,065	W	2	Fair.
	47	2	0	47	57	29,88	55		W	2	Fair.
4	33	7	0	33	53	30,24	65		W	1	Fair.
	41	2	0	41	54	30,34	64		SW	1	Fine.
5	37	7	0	43	53	30,17	65		SW	1	Cloudy.
	51	2	0	48	55	30,13	65		SW	1	Cloudy.
6	37	7	0	41	53	29,96	63		SW	2	Cloudy.
	51	2	0	51	55	29,78	64		SW	2	Cloudy.
7	48	7	0	48	54	29,78	67	0,020	SW	2	Cloudy.
	50	2	0	49	57	29,90	55		W	1	Cloudy.
8	40	7	0	40	54	29,84	66	0,295	SW	1	Rain.
	42	2	0	42	55	29,77	65		SW	1	Rain.
9	32	7	0	32	53	29,74	63	0,155	WSW	1	Fine.
	40	2	0	40	54	29,83	58		NW	1	Fair.
10	29	7	0	29	51	30,00	61		W	1	Fine.
	35	2	0	35	53	30,05	56		NW	1	Fine.
11	28	7	0	28	49	29,97	62		W	1	Cloudy.
	41	2	0	41	52	29,77	63		W	1	Cloudy.
12	30	7	0	31	48	29,23	72	0,175	NE	2	Snow. [much wind last night.
	31	2	0	31	51	29,52	70		NE	2	Snow.
13	24	7	0	29	47	29,66	63		NE	2	Snow.
	33	2	0	33	50	29,80	61		N	1	Fair.
14	24	7	0	25	47	29,97	64		NE	1	Cloudy.
	31	2	0	31	51	30,08	62		N	1	Fine.
15	18	7	0	20	45	30,18	64		W	1	Fair.
	38	2	0	33	48	30,02	66		SSW	1	Snow.
16	33	7	0	34	47	29,87	70		WNW	1	Fair.
	42	2	0	42	50	29,87	63		NW	1	Fair.

METEOROLOGICAL JOURNAL
for February, 1808.

1808	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°		°				
Feb. 17	30	7	0	32	47	30,04	66		NE	1	Cloudy.
	40	2	0	40	49	30,08	62		W	1	Cloudy.
18	38	7	0	43	48	29,98	73		W	1	Cloudy.
	47	2	0	45	51	30,00	72		NW	1	Cloudy.
19	35	7	0	35	49	30,21	64		NNE	1	Fair.
	42	2	0	42	52	30,29	60		NE	1	Fair.
20	31	7	0	31	48	30,46	65		ENE	1	Fair.
	39	2	0	39	52	30,48	60		E	1	Hazy.
21	29	7	0	29	48	30,52	66		NE	1	Fine.
	40	2	0	39	52	30,52	62		NE	1	Fine.
22	31	7	0	31	48	30,46	67		NE	1	Fine.
	41	2	0	40	53	30,47	62		NE	1	Fair.
23	33	7	0	33	49	30,41	66		NE	1	Cloudy.
	39	2	0	39	51	30,36	63		NE	1	Cloudy.
24	35	7	0	35	49	30,44	65		NE	2	Rain.
	41	2	0	41	52	30,51	58		NE	2	Fair.
25	31	7	0	31	48	30,71	58		NE	2	Fair.
	38	2	0	37	51	30,72	53		NE	1	Fair.
26	29	7	0	31	48	30,64	57		WNW	1	Cloudy.
	38	2	0	37	52	30,50	57		NW	1	Fair.
27	38	7	0	40	50	30,40	67		NW	1	Cloudy.
	48	2	0	48	54	30,38	56		W	1	Fair.
28	35	7	0	35	51	30,38	66		WSW	1	Fair.
	48	2	0	48	54	30,35	58		WNW	1	Fair.
29	46	7	0	47	52	30,12	66		WNW	1	Cloudy.
	52	2	0	52	55	30,14	65		NW	1	Cloudy.

METEOROLOGICAL JOURNAL
for March, 1808.

1808	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Mar. 1	47	7	0	47	54	30,33	71		NW	1	Cloudy.
	52	2	0	52	56	30,38	66		N	1	Cloudy.
2	47	7	0	47	55	30,34	63		NW	1	Cloudy.
	52	2	0	52	57	30,34	57		NW	1	Cloudy.
3	46	7	0	46	56	30,36	66		NW	1	Cloudy.
	51	2	0	51	57	30,35	54		NNW	1	Cloudy.
4	37	7	0	37	56	30,44	64		NNE	1	Fair.
	54	2	0	54	60	30,46	56		NE	1	Fair.
5	37	7	0	37	56	30,48	65		E	1	Cloudy.
	44	2	0	44	58	30,44	60		E	1	Fair.
6	32	7	0	32	54	30,45	64		NE	1	Fine.
	47	2	0	46	57	30,42	56		NNE	2	Fair.
7	35	7	0	35	53	30,38	60		NE	2	Cloudy.
	43	2	0	43	55	30,32	52		NE	2	Fair.
8	34	7	0	35	53	30,28	60		ENE	2	Cloudy.
	40	2	0	39	56	30,28	57		ENE	2	Fine.
9	33	7	0	33	53	30,34	60		NE	2	Cloudy.
	43	2	0	43	56	30,34	56		NE	1	Fair.
10	37	7	0	37	52	30,34	66		NE	1	Cloudy.
	40	2	0	40	53	30,38	60		ENE	1	Cloudy.
11	32	7	0	34	52	30,40	60		ENE	1	Fair.
	44	2	0	44	55	30,34	66		NE	1	Cloudy.
12	35	7	0	35	52	30,30	65		NE	1	Cloudy.
	40	2	0	40	54	30,26	60		NE	1	Cloudy.
13	38	7	0	38	52	30,18	66		NE	1	Cloudy.
	43	2	0	43	54	30,17	58		NE	1	Cloudy.
14	37	7	0	37	52	30,12	63		NE	1	Cloudy.
	42	2	0	42	54	30,07	60		ENE	1	Cloudy.
15	36	7	0	37	51	30,00	60		ENE	1	Cloudy.
	44	2	0	44	55	29,97	57		E	1	Fine.
16	35	7	0	36	52	30,00	65		E	1	Cloudy.
	44	2	0	43	55	29,98	56		E	1	Fair

METEOROLOGICAL JOURNAL
for March, 1808.

1808	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Mar. 17	31	7	0	33	51	30,00	60		NE	2	Cloudy.
	37	2	0	36	54	30,03	54		NE	1	Fair.
18	31	7	0	33	51	29,98	63		NE	2	Snow.
	37	2	0	35	53	29,90	60		NE	2	Cloudy.
19	30	7	0	33	51	29,76	60		ESE	2	Snow.
	38	2	0	38	53	29,65	58		ESE	2	Cloudy.
20	37	7	0	40	51	29,51	71	0,135	E	1	Cloudy.
	47	2	0	47	53	29,57	73		E	1	Cloudy.
21	39	7	0	39	51	29,73	73	0,032	E	1	Rain.
	42	2	0	42	53	29,80	70		NE	1	Cloudy.
22	38	7	0	38	51	29,93	65		NE	1	Cloudy.
	44	2	0	43	54	29,96	57		NE	2	Hazy.
23	30	7	0	31	50	29,98	62		NE	2	Fair.
	41	2	0	40	54	29,96	53		NE	2	Fair.
24	34	7	0	35	50	29,94	61		NE	2	Sleet.
	41	2	0	39	52	29,91	52		NE	1	Cloudy.
25	26	7	0	27	50	29,97	62		NE	1	Fair.
	40	2	0	40	53	30,06	55		NE	1	Fine.
26	31	7	0	33	51	29,98	67		NE	1	Cloudy.
	46	2	0	45	54	30,00	63		E	1	Cloudy.
27	37	7	0	37	52	30,07	62		NE	2	Cloudy.
	39	2	0	37	53	30,10	58		NE	2	Cloudy.
28	34	7	0	36	50	30,10	60		ENE	1	Fair.
	40	2	0	40	54	30,09	47		NE	2	Fair.
29	29	7	0	31	50	30,14	58		NE	1	Fair.
	43	2	0	43	54	30,13	48		ENE	1	Fine.
30	32	7	0	33	51	30,04	55		NE	2	Cloudy.
	40	2	0	39	52	30,01	51		NE	1	Cloudy.
31	35	7	0	35	50	29,96	63		NNE	2	Sleet.
	43	2	0	43	53	29,93	53		N	1	Cloudy.

METEOROLOGICAL JOURNAL
for April, 1808.

1808	Sin's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.		Inches.			
Apr. 1	32	7	0	33	50	29,78	63		NW	1	Fair.
	43	2	0	42	53	29,65	50		NW	1	Cloudy.
2	27	7	0	29	50	29,84	58		N	2	Fair.
	44	2	0	44	53	29,92	46		NW	1	Fair.
3	30	7	0	33	50	29,90	61		SW	2	Snow.
	50	2	0	48	53	29,83	62		SW	2	Cloudy.
4	40	7	0	42	50	29,72	60		S	2	Cloudy.
	49	2	0	47	52	29,55	66		S	2	Rain.
5	47	7	0	52	53	29,37	72	0,220	SSW	2	Cloudy.
	55	2	0	55	56	29,22	68		SSW	2	Rain.
6	49	7	0	52	54	29,62	73	0,185	SSW	2	Rain.
	56	2	0	56	57	29,69	68		S	2	Cloudy.
7	51	7	0	52	56	29,82	72	0,025	SSW	2	Rain.
	57	2	0	56	58	29,87	66		SSW	2	Cloudy.
8	41	7	0	41	57	29,63	67	0,040	NW	2	Rain.
	52	2	0	51	58	29,88	57		NW	2	Fair.
9	37	7	0	40	56	30,28	63		W	1	Fair.
	53	2	0	53	58	30,28	51		W	1	Fair.
10	43	7	0	44	55	30,29	64		WNW	1	Fair.
	56	2	0	56	58	30,28	51		NW	1	Cloudy.
11	47	7	0	48	57	30,26	54		W	1	Cloudy.
	58	2	0	57	58	30,24	48		W	1	Cloudy.
12	45	7	0	47	56	30,07	61		W	1	Fair.
	54	2	0	51	58	30,14	58		N	2	Rain.
13	40	7	0	41	57	30,32	62	0,032	W	1	Cloudy.
	58	2	0	57	59	30,27	51		WNW	1	Fair.
14	43	7	0	45	57	30,22	60		W	1	Fair.
	64	2	0	64	60	30,14	48		W	1	Fair.
15	45	7	0	48	59	30,07	60		W	1	Fair.
	63	2	0	63	62	30,00	50		W	1	Fine.
16	39	7	0	43	59	30,12	54		NNE	2	Fair.
	54	2	0	54	60	30,08	47		NNE	1	Fair.

METEOROLOGICAL JOURNAL
for April, 1808.

1808	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Rr.	Weather.
Apr. 17	35	7	0	38	57	30.20	57		SW	1	Fine.
	53	2	0	52	59	30.18	43		W	2	Cloudy.
18	33	7	0	37	56	30.06	56		WNW	2	Fair.
	48	2	0	46	58	29.95	46		NW	2	Cloudy.
19	36	7	0	41	54	29.[illegible]	57		S	2	Cloudy.
	45	2	0	37	55	29.[illegible]	60		NW	2	Snow.
20	32	7	0	34	53	29.[illegible]	60	0.075	SW	1	Hazy.
	49	2	0	47	55	29.[illegible]	50		E	1	Cloudy.
21	40	7	0	46	53	29.[illegible]	67	0.260	SSE	2	Cloudy.
	48	2	0	42	55	29.[illegible]	62		WNW	2	Cloudy.
22	36	7	0	37	53	29.[illegible]	63		WSW	2	Fair.
	48	2	0	45	55	29.[illegible]	60		SW	2	Rain.
23	39	7	0	42	53	29.[illegible]	65	0.245	WSW	1	Cloudy.
	50	2	0	48	55	29.[illegible]	57		WNW	1	Cloudy.
24	39	7	0	41	53	29.57	66	0.070	NE	1	Cloudy.
	50	2	0	48	56	29.[illegible]	60		NE	2	Rain.
25	36	7	0	37	53	29.[illegible]	62	0.016	NNE	2	Fair.
	46	2	0	44	54	29.[illegible]	65		N	2	Cloudy.
26	38	7	0	40	53	29.[illegible]	64		WNW	1	Cloudy.
	47	2	0	46	54	29.[illegible]	55		SW	1	Cloudy.
27	38	7	0	40	52	29.[illegible]	64		NE	1	Rain.
	45	2	0	43	54	29.[illegible]	67		N	1	Rain.
28	39	7	0	41	52	29.[illegible]	65	0.378	NE	1	Cloudy.
	48	2	0	48	55	29.[illegible]	60		NE	1	Cloudy.
29	38	7	0	40	53	29.[illegible]	61		NE	1	Cloudy.
	47	2	0	44	55	29.[illegible]	58		NE	1	Cloudy.
30	38	7	0	39	53	29.[illegible]	60		E	1	Hazy.
	52	2	0	51	55	29.[illegible]	50		S	1	Cloudy.

METEOROLOGICAL JOURNAL
for May, 1808.

1808	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.			Inches.		Inches.	Points.	Str.	
	°					°				
May 1	45	7 0	46	54	29,95	60		SW	1	Cloudy.
	62	2 0	61	57	29,98	52		S	1	Fair.
2	43	7 0	47	55	29,98	62		E	1	Fine.
	64	2 0	63	58	29,90	52		E	1	Fair.
3	54	7 0	57	57	29,83	57		E	1	Hazy.
	75	2 0	73	62	29,80	50		E	1	Fine.
4	52	7 0	55	59	29,83	57		E	1	Fair.
	75	2 0	75	64	29,83	47		E	1	Fine.
5	53	7 0	57	62	29,86	53		ENE	1	Hazy.
	72	2 0	70	64	29,77	48		E	1	Cloudy.
6	54	7 0	55	62	29,77	60		SSW	1	Cloudy.
	70	2 0	69	63	29,75	48		SSW	1	Fair.
7	56	7 0	58	63	29,62	58		S	1	Fine.
	69	2 0	69	64	29,56	51		S	2	Fair.
8	52	7 0	52	63	29,57	58	0,055	S	1	Rain.
	60	2 0	58	63	29,60	53		S	1	Cloudy.
9	48	7 0	50	61	29,55	57		SE	2	Cloudy.
	55	2 0	53	61	29,57	78		SSW	2	Rain.
10	46	7 0	48	60	29,80	61	0,230	S	2	Fine.
	60	2 0	60	60	29,75	58		S	2	Cloudy.
11	50	7 0	51	60	29,95	61		S	1	Fair.
	62	2 0	62	61	30,01	52		S	2	Cloudy.
12	49	7 0	52	60	30,17	64	0,070	S	2	Cloudy.
	65	2 0	63	62	30,21	55		S	2	Cloudy.
13	54	7 0	55	62	30,28	65		S	1	Cloudy.
	70	2 0	69	64	30,28	64		S	1	Hazy.
14	58	7 0	59	63	30,19	60		S	1	Cloudy.
	78	2 0	78	65	30,17	48		S	1	Fair.
15	58	7 0	61	64	30,19	60		SW	1	Fine.
	79	2 0	79	69	30,13	46		S	1	Fine.
16	65	7 0	66	67	30,00	50		S	1	Hazy.
	82	2 0	81	69	29,98	47		S	1	Hazy.

METEOROLOGICAL JOURNAL
for May, 1808.

1808	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Hygrometer. °	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
May 17	59	7	0	62	67	30,10	53		E	1	Hazy.
	75	2	0	75	69	30,05	48		NE	1	Hazy.
18	54	7	0	57	67	30,06	54		NE	1	Cloudy.
	59	2	0	58	63	30,14	54		NE	1	Cloudy.
19	42	7	0	46	64	30,16	56		NE	1	Hazy.
	59	2	0	57	64	30,21	50		ENE	1	Fair.
20	44	7	0	50	62	30,08	58		ENE	1	Fair.
	63	2	0	62	63	29,96	48		E	1	Fair.
21	53	7	0	54	63	29,80	61	0,105	E	1	Rain.
	65	2	0	63	63	29,76	56		SW	1	Cloudy.
22	54	7	0	56	63	29,61	67	0,160	SW	1	Cloudy.
	68	2	0	65	63	29,55	57		SSW	1	Cloudy.
23	53	7	0	54	62	29,65	60	0,185	SW	1	Cloudy.
	67	2	0	64	63	29,72	52		SW	2	Fair.
24	48	7	0	51	61	29,84	60		SW	2	Fine.
	65	2	0	64	62	29,90	50		SW	2	Cloudy.
25	52	7	0	55	61	29,95	54		N	1	Cloudy.
	72	2	0	70	64	29,91	52		ESE	1	Fair.
26	56	7	0	57	63	29,74	64	0,093	E	1	Rain.
	65	2	0	64	64	29,68	65		E	1	Rain.
27	54	7	0	55	63	29,76	62	0,180	S	1	Cloudy.
	63	2	0	61	62	29,80	61		S	1	Rain.
28	50	7	0	52	62	29,98	63	0,045	SSW	2	Fair.
	69	2	0	68	63	30,05	50		SSW	1	Fair.
29	52	7	0	55	62	30,17	58		SSE	1	Cloudy.
	66	2	0	65	62	30,20	58		S	1	Cloudy.
30	55	7	0	57	62	30,22	63		S	1	Fair.
	75	2	0	74	64	30,16	51		S	1	Fair.
31	56	7	0	60	63	30,03	60		NE	1	Hazy.
	76	2	0	74	66	29,87	51		E	1	Hazy.

METEOROLOGICAL JOURNAL

for June, 1808.

1808	Six's Therm. least and greatest Heat.	Time. H.	Time. M	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
June 1	60	7	0	60	64	29,82	63	0,035	N	1	Cloudy.
	63	2	0	62	64	29,91	56		NW	1	Cloudy.
2	48	7	0	52	63	30,02	60	0,035	WSW	1	Fine.
	69	2	0	69	64	30,00	47		SW	1	Fine.
3	51	7	0	54	62	29,90	57		S	2	Cloudy.
	66	2	0	65	63	29,86	47		S	2	Fair.
4	56	7	0	57	63	29,70	57		ESE	1	Hazy.
	63	2	0	62	63	29,63	58		ESE	1	Rain.
5	52	7	0	55	62	29,68	60	0,043	S	2	Fair.
	65	2	0	64	63	29,68	52		S	2	Cloudy.
6	49	7	0	51	61	29,71	61		SW	1	Cloudy.
	59	2	0	58	60	29,78	61		W	1	Cloudy.
7	50	7	0	52	60	29,83	58		W	1	Cloudy.
	66	2	0	59	61	29,86	53		WSW	1	Cloudy.
8	50	7	0	53	61	29,84	59		WSW	2	Fair.
	65	2	0	65	62	29,85	50		WNW	1	Fair.
9	52	7	0	53	60	29,65	64	0,035	SW	1	Rain.
	60	2	0	58	61	29,65	58		WSW	1	Rain.
10	52	7	0	53	60	29,85	65	0,140	NNE	2	Cloudy.
	63	2	0	62	62	29,94	55		NNE	2	Fair.
11	49	7	0	49	60	30,05	64		SW	1	Hazy.
	72	2	0	72	62	30,04	47		NW	1	Fair.
12	51	7	0	55	61	30,18	58		N	1	Cloudy.
	66	2	0	65	62	30,19	53		NE	1	Cloudy.
13	52	7	0	55	61	30,16	63		SW	1	Fair.
	72	2	0	70	63	30,12	52		W	1	Cloudy.
14	54	7	0	57	62	30,07	57		SW	1	Fair.
	72	2	0	70	64	29,98	50		SSW	2	Fair.
15	56	7	0	58	62	29,90	57	0,062	WNW	1	Fair.
	67	2	0	63	62	29,95	51		NW	1	Cloudy.
16	49	7	0	53	62	30,11	58		WNW	1	Fine.
	66	2	0	65	62	30,13	50		WNW	1	Fair.

METEOROLOGICAL JOURNAL

for June, 1808.

1808	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°		°				
June 17	55	7	0	57	62	30,15	60		SW	1	Cloudy.
	67	2	0	64	62	30,08	51		SW	1	Cloudy.
18	60	7	0	60	62	30,05	64		SW	1	Cloudy.
	76	2	0	75	65	30,06	55		W	1	Fair.
19	58	7	0	59	64	30,11	62		SW	1	Fair.
	76	2	0	75	66	30,08	50		W	1	Fair.
20	62	7	0	61	65	30,08	60		SW	1	Fine.
	75	2	0	73	66	30,03	50		N	1	Cloudy.
21	58	7	0	60	65	30,01	60		E	1	Cloudy.
	70	2	0	70	67	29,96	55		E	1	Cloudy.
22	57	7	0	58	65	29,83	55		E	1	Fair.
	70	2	0	70	67	29,77	53		E	1	Fine.
23	56	7	0	57	65	29,72	65	0,290	SW	1	Rain.
	69	2	0	68	66	29,77	50		S	2	Fair.
24	53	7	0	56	65	29,90	57		NE	1	Fair.
	71	2	0	70	65	29,91	48		NE	1	Hazy.
25	54	7	0	57	65	30,05	60		SW	1	Fair.
	73	2	0	72	67	30,04	54		NE	1	Fair.
26	54	7	0	57	65	30,10	57		E	1	Fine.
	75	2	0	75	67	30,07	49		NE	1	Fine.
27	56	7	0	56	65	30,08	58		NE	1	Cloudy.
	68	2	0	66	66	30,08	53		NE	1	Fair.
28	53	7	0	54	63	30,11	57		NE	1	Cloudy.
	61	2	0	60	64	30,11	55		NE	1	Cloudy.
29	54	7	0	56	64	30,17	59		NE	1	Cloudy.
	75	2	0	72	66	30,17	51		E	1	Fine.
30	54	7	0	57	65	30,26	60		NE	1	Cloudy.
	70	2	0	70	67	30,25	53		ENE	2	Fair.

METEOROLOGICAL JOURNAL
for July, 1808.

1808	Sir's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°		°				
July 1	52	7	0	57	65	30,20	57		NE	1	Cloudy.
	70	2	0	70	67	30,13	52		E	1	Fine.
2	51	7	0	57	63	30,10	58		ENE	2	Fair.
	69	2	0	68	67	30,05	51		NE	1	Fair.
3	54	7	0	57	64	30,05	57		NE	1	Fine.
	71	2	0	71	66	30,01	50		NE	1	Fair.
4	50	7	0	54	64	30,04	63	0,080	NE	1	Hazy.
	70	2	0	69	66	29,97	47		W	1	Fair.
5	50	7	0	54	64	29,99	57		NE	1	Fine.
	67	2	0	67	64	30,03	47		NE	1	Fair.
6	55	7	0	59	63	30,12	58		WNW	1	Cloudy.
	69	2	0	67	64	30,18	54		WNW	1	Cloudy.
7	58	7	0	58	64	30,19	57		SSW	1	Fine.
	75	2	0	75	66	30,13	48		S	1	Fine.
8	56	7	0	59	64	30,07	55		SSW	2	Fine.
	77	2	0	77	67	30,00	47		S	2	Fair.
9	60	7	0	62	65	30,04	64		W	1	Cloudy.
	72	2	0	71	67	30,10	56		N	1	Cloudy.
10	58	7	0	59	65	30,10	62		S	1	Fair.
	74	2	0	73	67	30,10	53		S	2	Cloudy.
11	60	7	0	61	66	30,18	65		SW	1	Cloudy.
	80	2	0	78	68	30,18	50		S	1	Fine.
12	62	7	0	67	68	30,17	61		NE	1	Hazy.
	90	2	0	88	73	30,12	45		SE	1	Hazy.
13	67	7	0	72	72	30,03	55		NE	1	Fine.
	93½	2	0	92	76	30,00	45		S	1	Fine.
14	71	7	0	73	75	30,04	54		SW	1	Hazy.
	91	2	0	90	78	30,07	45		W	1	Hazy.
15	65	7	0	70	75	30,04	58		NE	2	Fine.
	79	2	0	78	77	30,00	54		E	2	Fine.
16	64	7	0	68	74	29,97	59		E	2	Fine.
	86	2	0	85	77	29,95	52		S	1	Hazy.

METEOROLOGICAL JOURNAL
for July, 1808.

1808	Six's Therm. least and greatest Heat.	Time, H.	Time, M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
July 17	61	7	0	64	73	30.04	63		SW	1	Cloudy.
	83	2	0	82	75	30.99	48		NNE	1	Fair.
18	64	7	0	67	75	30.13	56		N	1	Fine.
	83	2	0	83	76	30.05	47		E	1	Hazy.
19	64	7	0	68	74	29.98	56		E	1	Hazy.
	88	2	0	84	77	29.85	50		E	1	Fair.
20	61	7	0	63	72	29.86	57		SSW	1	Fine.
	75	2	0	75	73	29.81	50		S	2	Cloudy.
21	59	7	0	60	70	29.74	60	0,016	S	1	Fair.
	76	2	0	75	72	29.71	50		S	2	Fair.
22	59	7	0	60	68	29.76	60		SE	2	Cloudy.
	74	2	0	72	69	29.79	50		S	2	Fair.
23	63	7	0	64	69	29.84	57		E	1	Fine.
	78	2	0	77	72	29.84	50		SE	2	Fair.
24	64	7	0	64	70	29.84	60	0,046	S	1	Fine.
	76	2	0	70	71	29.78	61		NW	1	Rain.
25	62	7	0	65	70	29.76	62	0,440	W	1	Cloudy.
	72	2	0	69	71	29.76	60		W	1	Rain. [thunder.
26	60	7	0	63	69	29.82	66	0,430	SW	1	Cloudy.
	73	2	0	72	70	29.84	56		W	1	Cloudy.
27	60	7	0	62	68	29.85	64	0,190	NE	1	Cloudy.
	75	2	0	74	70	29.77	64		ESE	1	Cloudy.
28	59	7	0	61	68	29.48	77	0,943	N	1	Rain.
	66	2	0	66	69	29.51	69		NW	1	Rain.
29	62	7	0	63	68	29.65	70	0,313	WSW	1	Cloudy.
	75	2	0	73	69	29.70	53		S	1	Cloudy.
30	62	7	0	63	68	29.74	65		S	2	Cloudy.
	76	2	0	76	70	29.80	50		S	1	Fair.
31	60	7	0	62	68	29.85	62		S	1	Fine.
	80	2	0	80	71	29.74	50		ESE	1	Fair.

METEOROLOGICAL JOURNAL

for August, 1808.

1808	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.	°	Inches.	Points.	Str.	
Aug. 1	62	7 0	64	68	29,64	64	0,352	S	2	Fair.
	71	2 0	69	69	29,57	61		S	2	Rain.
2	59	7 0	60	69	29,80	67	0,160	SW	1	Cloudy.
	76	2 0	75	70	29,88	52		NW	2	Fair.
3	59	7 0	61	67	30,09	56		W8W	1	Cloudy.
	72	2 0	72	69	30,14	51		W	1	Cloudy.
4	58	7 0	59	67	30,10	63		SW	1	Fine.
	77	2 0	77	70	30,02	48		S	1	Fine.
5	58	7 0	59	68	29,91	60		S	1	Fine.
	80	2 0	78	70	29,82	48		S	1	Fine.
6	64	7 0	65	69	29,74	60		S	1	Fair.
	77	2 0	75	71	29,74	52		S	2	Cloudy.
7	61	7 0	62	68	29,85	63		S	2	Cloudy.
	71	2 0	70	69	29,88	57		S	2	Cloudy.
8	63	7 0	63	68	29,83	70	0,295	S	1	Rain.
	75	2 0	75	70	29,76	51		S	2	Fair.
9	61	7 0	61	68	29,63	64	0,182	E	1	Cloudy.
	72	2 0	71	69	29,59	55		E	1	Cloudy.
10	59	7 0	62	68	29,77	65	0,016	W	1	Cloudy.
	73	2 0	72	69	29,78	53		W	1	Fair.
11	60	7 0	62	67	29,74	59		SW	1	Cloudy.
	74	2 0	74	69	29,72	48		S	1	Fair.
12	58	7 0	58	86	29,81	59		S	1	Hazy.
	76	2 0	76	70	29,83	49		S	1	Fine.
13	61	7 0	63	67	29,75	60		S	2	Cloudy.
	69	2 0	68	68	29,67	64		S	2	Rain.
14	60	7 0	61	67	29,72	64	0,045	S	1	Rain.
	73	2 0	72	68	29,66	51		W	2	Cloudy.
15	58	7 0	60	65	29,64	62		SSW	2	Fair.
	73	2 0	73	68	29,68	50		W	2	Fair.
16	55	7 0	58	65	29,87	61	0,052	SW	2	Cloudy.
	72	2 0	72	68	29,84	52		SW	1	Cloudy.

METEOROLOGICAL JOURNAL

for August, 1808.

1808	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Aug. 17	60	7	0	61	66	29,88	60		WSW	1	Cloudy.
	71	2	0	70	67	29,95	50		W	1	Cloudy.
18	59	7	0	61	65	30,04	54		E	1	Hazy.
	71	2	0	71	67	30,05	50		NE	1	Fine.
19	60	7	0	62	66	30,11	58		W	1	Cloudy.
	72	2	0	72	67	30,13	48		NW	1	Cloudy.
20	56	7	0	58	65	30,18	61		N	1	Hazy.
	72	2	0	72	68	30,19	50		E	1	Fine.
21	58	7	0	60	66	30,21	62		E	1	Fine.
	76	2	0	76	68	30,21	53		ENE	1	Fair.
22	60	7	0	62	66	30,20	62		NE	1	Fair.
	72	2	0	71	68	30,18	57		E	1	Fair.
23	56	7	0	60	65	30,17	58		NE	1	Hazy.
	69	2	0	69	67	30,15	52		ENE	2	Cloudy.
24	57	7	0	59	65	30,12	57		NE	1	Fine.
	71	2	0	71	67	30,12	50		NE	1	Fair.
25	55	7	0	58	64	30,12	66		NE	1	Hazy.
	71	2	0	71	67	30,05	50		NE	1	Fair.
26	55	7	0	56	65	29,85	63		SW	1	Fine.
	73	2	0	73	68	29,71	50		SW	1	Fair.
27	58	7	0	60	65	29,56	64		SW	2	Cloudy.
	68	2	0	67	66	29,62	50		NW	1	Cloudy.
28	51	7	0	53	63	29,68	60	0,020	SW	1	Fine.
	68	2	0	68	66	29,77	45		WSW	1	Fine.
29	52	7	0	55	64	29,91	60		SW	1	Fine.
	71	2	0	71	67	29,90	50		SE	1	Fair.
30	61	7	0	64	67	29,75	66	0,018	S	2	Cloudy.
	74	2	0	73	68	29,68	50		S	2	Cloudy.
31	57	7	0	58	65	29,61	65	0,075	S	1	Fine.
	68	2	0	66	66	29,55	58		S	2	Rain.

METEOROLOGICAL JOURNAL
for September, 1808.

1808	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.			Inches.		Inches.	Points.	Str.	
	°			°	°		°				
Sep. 1	57	7	0	58	65	29,59	63	0,095	S	2	Fine. [much wind last night.
	69	2	0	68	67	29,66	57		S	2	Cloudy.
2	55	7	0	56	65	29,82	63	0,245	S	2	Fair.
	68	2	0	64	64	29,83	63		S	1	Rain.
3	55	7	0	56	64	29,84	66	0,395	SW	1	Cloudy.
	68	2	0	67	66	29,86	55		WNW	1	Cloudy.
4	57	7	0	58	64	29,83	69	0,045	SW	1	Cloudy.
	67	2	0	66	66	29,85	63		SW	1	Cloudy.
5	52	7	0	54	63	29,82	62		S	1	Cloudy.
	66	2	0	64	64	29,72	60		S	2	Cloudy.
6	53	7	0	54	65	29,72	63	0,073	S	2	Fair.
	67	2	0	66	65	29,72	53		SW	1	Fair.
7	54	7	0	56	63	29,78	67	0,063	S	2	Cloudy.
	67	2	0	67	64	29,76	60		S	2	Cloudy.
8	57	7	0	58	63	29,58	64	0,055	S	1	Cloudy.
	67	2	0	67	64	29,51	53		S	2	Fair.
9	53	7	0	56	63	29,31	62		SE	2	Cloudy.
	64	2	0	63	64	29,30	60		S	2	Fair.
10	56	7	0	57	63	29,32	65	0,373	ESE	1	Rain.
	63	2	0	62	64	29,33	65		SE	1	Rain.
11	54	7	0	54	63	29,44	69	0,570	SW	1	Cloudy.
	64	2	0	59	63	29,53	63		W	1	Rain.
12	54	7	0	54	62	29,63	70	0,586	SW	1	Cloudy.
	64	2	0	62	63	29,65	64		SW	1	Rain.
13	54	7	0	56	62	29,69	72	0,480	NE	1	Rain.
	65	2	0	64	63	29,71	60		SE	1	Rain.
14	55	7	0	57	62	29,76	73	0,285	NW	1	Cloudy.
	66	2	0	66	64	29,82	59		NE	1	Fair.
15	58	7	0	60	63	30,00	71		NNE	1	Cloudy.
	71	2	0	71	65	30,11	53		NE	1	Fair.
16	56	7	0	56	63	30,27	70		NE	1	Cloudy.
	65	2	0	65	65	30,28	51		E	1	Fair.

METEOROLOGICAL JOURNAL
for September, 1808.

1808	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Sep. 17	52	7	0	53	63	30,24	63		NE	1	Fine.
	65	2	0	65	64	30,16	51		NE	2	Fair.
18	54	7	0	54	63	29,96	68		ENE	1	Cloudy.
	68	2	0	68	65	29,88	55	0,180	E	1	Fair.
19	59	7	0	59	63	29,86	73		NW	1	Rain.
	67	2	0	66	66	29,93	53		W	1	Fair.
20	52	7	0	53	63	30,18	67		SW	1	Fine.
	68	2	0	66	65	30,25	53		W	1	Fair.
21	51	7	0	52	64	30,34	67		W	1	Foggy.
	66	2	0	66	65	30,31	58		S	1	Fine.
22	51	7	0	52	64	30,16	66		S	1	Foggy.
	67	2	0	67	64	29,97	57		S	1	Cloudy.
23	53	7	0	53	63	29,64	65	0,335	W	1	Fair.
	59	2	0	58	63	29,66	63		N	1	Cloudy.
24	45	7	0	46	61	29,94	62		N	1	Fine.
	55	2	0	54	62	29,98	53		N	1	Fair.
25	49	7	0	51	61	30,05	61		WSW	1	Cloudy.
	60	2	0	59	61	30,09	60		NE	1	Cloudy.
26	48	7	0	49	60	30,08	62		E	1	Fine.
	63	2	0	63	63	30,01	56		SSW	1	Fine.
27	48	7	0	48	61	29,94	56		NW	1	Cloudy.
	56	2	0	56	62	29,88	50		N	1	Fine.
28	42	7	0	44	60	29,76	60		WNW	1	Cloudy.
	52	2	0	52	60	29,57	51		SW	1	Hazy.
29	40	7	0	40	57	29,40	56		N	1	Cloudy.
	52	2	0	50	58	29,40	57		N	1	Rain.
30	39	7	0	39	57	29,51	63		NW	1	Cloudy.
	52	2	0	51	60	29,58	56		NW	1	Fair.

METEOROLOGICAL JOURNAL
for October, 1808.

1808	Six's Therm. least and greatest Heat.	Time.		Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H.	M.	°	°	Inches.	°	Inches.	Points.	Str.	
Oct. 1	41	7	0	41	57	29,85	60	0,053	NW	1	Fair.
	52	2	0	52	57	29,91	50		NW	1	Fair.
2	39	7	0	43	56	29,82	60		SW	1	Cloudy.
	56	2	0	54	58	29,61	67		SE	2	Rain.
3	48	7	0	48	57	29,90	71	0,158	W	1	Rain.
	56	2	0	56	59	30,06	60		W	1	Cloudy.
4	45	7	0	46	57	30,19	66		WSW	1	Fair.
	57	2	0	57	59	30,21	66		SW	1	Rain.
5	48	7	0	48	57	30,20	71	0,122	SW	1	Foggy.
	62	2	0	61	60	30,05	60		S	1	Cloudy.
6	46	7	0	48	57	30,02	69		E	1	Fine.
	63	2	0	62	60	30,01	61		WSW	1	Fair.
7	50	7	0	51	58	30,06	63		N	1	Cloudy.
	57	2	0	56	60	29,91	62		SSW	1	Cloudy.
8	48	7	0	48	58	29,34	62	0,365	W	2	Fine.
	53	2	0	52	59	29,30	53		WNW	2	Cloudy.
9	41	7	0	41	57	29,73	60	0,016	WNW	2	Fine.
	52	2	0	51	58	29,82	50		NW	2	Fine.
10	43	7	0	49	57	29,74	68		W	1	Cloudy.
	58	2	0	57	59	29,85	54		NW	2	Cloudy.
11	43	7	0	43	57	30,04	65		W	1	Fair.
	58	2	0	57	60	29,96	56		SW	1	Fine.
12	45	7	0	45	58	29,95	62	0,133	W	1	Fine.
	53	2	0	52	59	29,97	56		WNW	1	Fair.
13	38	7	0	38	57	30,07	58		NW	1	Fine.
	48	2	0	48	59	30,11	50		NW	1	Fair.
14	44	7	0	47	56	29,59	67	0,057	S	2	Rain.
	57	2	0	54	59	29,39	53		WNW	2	Cloudy.
15	40	7	0	43	56	29,18	65	0,065	SW	1	Rain.
	50	2	0	50	57	29, 20	60		W	1	Rain.
16	43	7	0	43	56	29,55	65	0,061	W	1	Cloudy.
	51	2	0	49	56	29,53	60		SW	1	Rain.

METEOROLOGICAL JOURNAL
for October, 1808.

1808	Six's Therm. least and greatest Heat.	Time. H.	M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Str.	Weather.
	°			°	°	Inches.	°	Inches.			
Oct. 17	41	7	0	41	54	29,56	62	0,047	W	1	Fair.
	48	2	0	47	56	29,61	55		NW	2	Fair.
18	40	7	0	41	54	29,77	64		WSW	1	Fair.
	53	2	0	52	57	29,72	55		W	1	Fair.
19	40	7	0	41	54	29,50	63	0,072	SW	1	Fine.
	50	2	0	50	55	29,48	55		W	1	Cloudy.
20	43	7	0	43	53	29,69	63		SW	1	Fair.
	53	2	0	53	56	29,73	54		NW	1	Cloudy.
21	47	7	0	52	55	29,30	73	0,305	SSW	1	Rain.
	53	2	0	52	59	29,48	53		WNW	2	Fair.
22	42	7	0	44	55	29,53	63		SW	1	Fair.
	50	2	0	50	57	29,57	53		W	1	Fine.
23	35	7	0	35	54	29,81	63		SW	1	Fine.
	52	2	0	51	56	29,78	56		SSW	1	Hazy.
24	48	7	0	48	55	29,32	75	0,465	SW	1	Rain. [much wind last night.
	55	2	0	54	57	29,43	55		SW	2	Cloudy.
25	40	7	0	40	54	29,71	68		SW	1	Fine.
	54	2	0	53	56	29,61	59		S	2	Hazy.
26	39	7	0	39	56	29,22	67	0,335	SW	1	Cloudy.
	56	2	0	54	58	29,28	56		SW	2	Fair.
27	43	7	0	44	55	29,42	64		S	2	Cloudy.
	52	2	0	51	57	29,45	61		S	2	Rain.
28	44	7	0	48	56	29,41	70	0,515	S	2	Rain.
	53	2	0	53	58	29,48	62		S	2	Fair.
29	46	7	0	46	56	29,67	68	0,318	SE	1	Cloudy.
	52	2	0	52	58	29,76	70		S	1	Cloudy.
30	44	7	0	44	56	30,03	70	0,112	S	1	Rain.
	53	2	0	52	57	30,11	67		NE	1	Cloudy.
31	45	7	0	45	56	30,31	73		NE	1	Cloudy.
	52	2	0	52	58	30,24	67		NE	1	Cloudy.

METEOROLOGICAL JOURNAL
for November, 1808.

1808	Six's Therm. least and greatest Heat.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.	°	Inches.	Points.	Str.	
Nov. 1	48	7 0	48	56	30,29	68		NE	1	Cloudy.
	52	2 0	51	58	30,24	63		NE	1	Cloudy.
2	48	7 0	48	56	30,18	65		ENE	1	Cloudy.
	52	2 0	52	58	30,13	60		ENE	1	Cloudy.
3	48	7 0	48	56	30,05	68		ENE	2	Cloudy.
	49	2 0	48	58	30,07	63		E	2	Cloudy.
4	44	7 0	44	56	30,22	58		E	1	Fair.
	48	2 0	48	58	30,18	56		E	1	Cloudy.
5	40	7 0	40	55	29,91	60		NE	1	Cloudy.
	44	2 0	43	56	29,89	57		ESE	1	Fair.
6	31	7 0	32	53	29,92	62		NE	1	Fine.
	44	2 0	44	56	29,82	60		E	1	Fine.
7	38	7 0	41	53	29,72	68		ENE	1	Cloudy.
	46	2 0	46	55	29,68	63		E	1	Cloudy.
8	44	7 0	44	53	29,66	70		NE	1	Cloudy.
	51	2 0	51	55	29,62	70		NE	1	Cloudy.
9	46	7 0	46	54	29,56	73	0,085	N	1	Cloudy.
	53	2 0	53	55	29,63	72		NE	1	Cloudy.
10	47	7 0	47	55	29,76	73		NE	1	Fair.
	52	2 0	52	58	29,73	65		NE	1	Fine.
11	47	7 0	47	57	29,85	62		NE	1	Cloudy.
	49	2 0	49	58	29,88	64		NE	1	Cloudy.
12	38	7 0	38	56	30,07	63		NE	1	Fair.
	45	2 0	44	56	30,12	57		E	1	Cloudy.
13	40	7 0	40	55	30,12	60		ENE	1	Cloudy.
	43	2 0	43	55	30,10	58		NE	1	Cloudy.
14	33	7 0	33	53	30,12	60		NE	1	Fine.
	38	2 0	38	54	30,11	60		NE	1	Hazy.
15	34	7 0	35	51	30,00	63		SW	1	Cloudy.
	47	2 0	47	53	29,92	65		S	1	Cloudy.
16	46	7 0	50	52	29,67	72		S	2	Cloudy.
	52	2 0	51	55	29,50	63		S	2	Fair.

METEOROLOGICAL JOURNAL
for November, 1808.

1808	Six't Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds. Points.	Winds. Str.	Weather.
	°			°	°	Inches.		Inches.			
Nov. 17	50	7	0	52	54	29,31	73	0,150	S	2	Cloudy.
	56	2	0	55	58	29,20	65		S	2	Fair.
18	48	7	0	48	56	28,80	70	0,095	S	2	Rain.
	50	2	0	50	57	28,74	64		S	2	Cloudy.
19	36	7	0	37	54	29,25	67	0,440	NE	1	Snow.
	44	2	0	44	56	29,54	60		WNW	1	Fair.
20	39	7	0	40	54	29,75	65		WSW	1	Cloudy.
	48	2	0	48	56	29,78	66		SSW	1	Cloudy.
21	36	7	0	52	55	29,75	71		SSW	2	Cloudy.
	56	2	0	54	57	29,85	58		WSW	1	Fair.
22	41	7	0	41	54	30,18	66		W	1	Fair.
	49	2	0	49	55	30,22	62		W	1	Cloudy.
23	47	7	0	48	54	30,13	70		W	1	Cloudy.
	54	2	0	54	57	30,13	66		SW	1	Cloudy.
24	47	7	0	47	55	30,18	64		N	1	Cloudy.
	50	2	0	49	57	30,22	57		N	1	Cloudy.
25	46	7	0	48	56	30,02	73	0,415	SW	1	Rain.
	55	2	0	55	59	29,95	69		SW	1	Cloudy.
26	46	7	0	48	55	29,98	68		WSW	1	Cloudy.
	56	2	0	56	59	29,89	67		SW	1	Cloudy.
27	52	7	0	52	56	29,55	72	0,060	SW	1	Cloudy.
	56	2	0	48	58	29,45	66		NW	1	Rain.
28	36	7	0	36	56	29,78	64		N	1	Fair.
	42	2	0	42	57	29,86	62		NW	1	Fine.
29	35	7	0	35	53	29,81	65		NW	1	Cloudy.
	42	2	0	38	55	29,72	65		S	1	Rain.
30	39	7	0	43	54	29,13	60	0,500	W	1	Fair.
	49	2	0	48	55	29,30	57		WNW	1	Fair.

METEOROLOGICAL JOURNAL

for December, 1808.

1808	Six's Therm. least and greatest Heat.	Time. H.	Time. M.	Therm. without.	Therm. within.	Barom. Inches.	Hygrometer.	Rain. Inches.	Winds. Points.	Winds. Str.	Weather.
	°			°	°		°				
Dec. 1	39	8	0	39	53	29,37	68		W	1	Fair.
	48	2	0	48	55	29,48	62		WSW	1	Fine.
2	44	8	0	44	55	29,20	66	0,175	W	2	Fair.
	50	2	0	50	56	29,24	61		W	2	Cloudy.
3	43	8	0	44	55	29,48	66		WSW	1	Fair.
	50	2	0	50	57	29,44	60		SW	1	Fine.
4	45	8	0	45	55	30,02	65		N	1	Cloudy.
	47	2	0	46	57	30,16	61		N	1	Fair.
5	36	8	0	40	53	30,30	63		NE	1	Cloudy.
	52	2	0	49	56	30,28	67		S	1	Cloudy.
6	49	8	0	49	55	30,03	71		SW	2	Cloudy.
	53	2	0	53	57	29,78	64		SW	2	Cloudy.
7	39	8	0	39	53	29,86	57	0,020	WNW	1	Fair.
	43	2	0	43	55	29,90	55		N	1	Fair.
8	38	8	0	40	52	29,95	55		WNW	2	Cloudy.
	43	2	0	43	54	29,99	56		NW	1	Cloudy.
9	41	8	0	42	53	29,88	64		N	1	Cloudy.
	45	2	0	44	56	29,94	62		NNE	1	Cloudy.
10	34	8	0	36	53	30,08	63		N	1	Cloudy.
	42	2	0	41	55	30,13	63		S	1	Fair.
11	37	8	0	37	53	30,30	66		WNW	1	Fair.
	42	2	0	42	54	30,30	68		NW	1	Cloudy.
12	34	8	0	38	52	30,28	68		WNW	1	Foggy.
	43	2	0	43	53	30,30	68		NW	1	Cloudy.
13	38	8	0	38	51	30,33	70		N	1	Cloudy.
	43	2	0	43	53	30,35	70		NW	1	Cloudy.
14	34	8	0	35	50	30,35	70		W	1	Cloudy.
	43	2	0	41	52	30,25	67		WSW	1	Cloudy.
15	37	8	0	38	50	30,07	66	0,045	NW	1	Cloudy.
	41	2	0	41	52	30,05	64		NNE	1	Fair.
16	35	8	0	36	50	30,10	70		NNE	1	Cloudy.
	37	2	0	37	52	30,06	60		NE	1	Fair.

METEOROLOGICAL JOURNAL,
for December, 1808.

1808	Six's Therm. least and greatest Mest.	Time.	Therm. without.	Therm. within.	Barom.	Hygrometer.	Rain.	Winds.		Weather.
		H. M.	°	°	Inches.		Inches.	Points.	Str.	
	°					°				
Dec. 17	32	8 0	33	50	29,86	67		NW	1	Foggy.
	40	2 0	39	52	29,52	67		W	1	Sleet.
18	24	8 0	25	46	29,83	60		N	2	Cloudy.
	32	2 0	30	50	29,65	60		N	2	Fine.
19	29	8 0	30	46	29,53	68		N	1	Cloudy.
	34	2 0	32	49	29,62	67		N	1	Snow.
20	27	8 0	27	45	29,67	67		N	1	Cloudy.
	31	2 0	31	48	29,77	65		N	1	Fair.
21	19	8 0	20	44	30,00	65		W	1	Fair.
	35	2 0	31	46	29,85	69		SSW	1	Cloudy.
22	31	8 0	31	43	29,24	68		W	1	Snow.
	34	2 0	34	45	29,07	69		W	1	Cloudy.
23	27	8 0	28	44	29,40	64		N	2	Cloudy.
	33	2 0	31	47	29,43	65		N	2	Cloudy.
24	30	8 0	31	46	29,46	70		NNE	1	Cloudy.
	33	2 0	30	47	29,55	67		E	2	Cloudy.
25	27	8 0	29	45	29,80	67		NNE	1	Cloudy.
	31	2 0	31	46	29,50	67		NNE	1	Foggy.
26	23	8 0	23	43	29,51	67		NE	1	Fair.
	31	2 0	31	46	29,50	68		ENE	1	Cloudy.
27	31	8 0	34	43	29,47	70		NE	1	Cloudy.
	36	2 0	36	46	29,48	72		NE	1	Cloudy.
28	36	8 0	38	43	29,00	73		NE	1	Cloudy.
	40	2 0	39	47	29,48	73		NE	1	Cloudy.
29	37	8 0	39	46	29,50	75	0,450	E	1	Rain.
	43	2 0	43	48	29,48	76		E	1	Cloudy.
30	39	8 0	40	47	29,49	74	0,043	E	1	Cloudy.
	45	2 0	44	50	29,50	74		E	1	Cloudy.
31	38	8 0	38	47	29,59	76		E	1	Cloudy.
	38	2 0	38	50	29,00	72		E	1	Cloudy.

1808.	Six's Therm. without.			Thermometer without.			Thermometer within.			Barometer.*			Hygrometer.			Rain.
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.
January	53	18	38,8	52	18	39,2	56	46	51,1	30,51	28,90	29,84	74	53	65,1	1,059
February	53	18	38,3	52	20	38,5	57	45	51,4	30,72	29,23	30,10	73	53	63,3	0,710
March	52	26	39,2	52	27	39,4	60	50	53,1	30,48	29,51	30,12	73	47	60,4	0,167
April	64	27	45,2	64	29	45,6	62	50	54,7	30,29	29,15	29,86	73	43	59,2	1,646
May	82	42	60,0	81	46	60,6	69	54	62,5	30,28	29,55	29,93	65	46	55,9	1,185
June	76	48	61,1	75	49	61,4	67	60	63,4	30,26	29,63	29,97	65	47	56,0	0,640
July	93,5	50	68,3	92	54	69,1	78	63	69,4	30,20	29,48	29,95	77	45	56,1	2,458
August	80	51	65,5	78	53	66,0	71	63	67,2	30,21	29,55	29,87	70	45	56,7	1,215
September	71	39	57,9	71	39	57,8	67	57	63,0	30,34	29,30	29,56	73	50	61,2	3,780
October	63	35	48,0	62	35	48,7	60	53	56,9	30,31	29,18	29,73	75	50	61,8	3,199
November	56	31	45,7	56	32	46,0	59	51	55,5	30,29	28,74	29,82	73	53	64,6	1,745
December	53	19	37,6	53	20	37,7	57	43	50,2	30,35	29,07	29,76	76	55	66,2	0,733
Whole year			50,5			50,8			58,2			29,87			60,5	18,475

* The quicksilver in the bason of the barometer, is 81 feet above the level of low water spring tides at Somerset-house.

Magnetic Needle,

June, 1808.

Variation,	-	24° 10′
Dip	- -	70° 1′